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THE JOHNS HOPKINS UNIVERSITY

Editor—HENRY E. SIGERIST

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Editor—HENRY E. SIGERIST

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PREFACE TO VOLUME IV

THE HISTORY OF MEDICINE *AND* THE HISTORY OF SCIENCE

AN OPEN LETTER TO GEORGE SARTON, EDITOR OF *Isis*

My dear colleague and friend:

You recently wrote a second preface to the 23rd volume of *Isis* entitled, "The History of Science Versus the History of Medicine." You are certainly justified in speaking authoritatively. Twenty-three volumes are a record of which you can justly be proud. Your daughter, *Isis*, born in hard years, nursed with infinite care, is no longer a child. She has grown and developed splendidly and we all love and admire her. It was good news to hear that she soon will have a brother-husband in *Osiris* and you may be assured that every new member in the family, be it Horus, Thoth or Sekhmet, will find a warm welcome among us. It is in all due modesty that I open the fourth volume of our Bulletin (what a prosaic, earthly name!) by answering you.

I feel that I have to write you not only in my own name, but in the name of my colleagues in the history of medicine against whom you seem to bear a grievance.

When I first read the title of your preface, I was terribly shocked. I immediately saw two sisters raised against each other, ready to pull each other's hair, an ugly sight.

I agree with you wholeheartedly in quite a few points. You are

certainly right when you state that much of the work done in the history of medicine is worthless, being written by ignorant amateurs. I would even go further and say that many of their papers and books are not only worthless but criminal. They perpetuate old errors and add new ones. They are the more dangerous as they are often written by eminent physicians—eminent in any but the historical field—which gives them great authority and popularity. These pseudo-historians do not realize that their action is dishonest. They would be appalled at the idea of publishing false laboratory reports and yet they do not hesitate to print erroneous historical statements. After all, it is but history and does not affect their career. Medical men trained in scientific methods do not know that historical research has exact methods also—that these methods impose an iron discipline upon the historian and that it takes infinitely more time and labour to master these methods than it does to learn the methods of biochemistry. They are not aware of the responsibility of the historian and act frivolously. I could at any time write a book on heart diseases or any other medical subject. I know how to handle books and could compile some kind of a readable textbook. But I also know that such a book based on second-hand sources would be utterly worthless. The situation is very serious indeed.

And yet, these amateur historians are nice cultured physicians. They are enthusiasts and I would hate to discourage them. After a day of hard work, instead of playing bridge, they delight in taking an old medical book from the shelves; they delve into the past, find relaxation and have a devil of a good time. Since they naturally want their friends to share their pleasure, they read papers in club meetings, publish them and sometimes write big books. Nothing pleases me more than to find a warm enthusiasm for the history of medicine and I am glad to say that it is much more frequently found in this country than in any other country abroad. A medical history club in a hospital, university or community is a cultural centre of great value to the profession.

How, then, is the situation to be handled? I know that we both agree in that the amateur has a very important part to play in historical research—provided he is aware of his limitations. It seems

to me that we have to distinguish between two things. The activities in clubs, where the members tell each other of their readings and findings, need not be looked at too critically. They are like amateur chambre-music performed for the enjoyment of the participants. It is different when papers are published, when they become literature influencing further research. Whoever publishes a paper or book must be aware of his responsibility. A wrong historical statement will not kill a patient as a wrong diagnosis may do but it can cause great harm nevertheless. The amateur should be careful in the selection of his topics. He should not write on Greek medicine unless he has a knowledge of Greek and is conversant with the methods and literature of classical philology. Nor should he write on mediaeval medicine unless he knows mediaeval Latin and has got a glimpse of the mediaeval spirit that is to be found not only in medical but in theological books. Whatever he does, he should go back to the original sources. And this can be done. The field is so vast. The local history has to be investigated and who could do it better than the physician who lives on the spot. Many doctors of old deserve to have their biographies written. Is there a more beautiful task than to resuscitate the life and achievements of a fellow-doctor even if he is not among the heroes of medicine? A specialist will naturally be interested in the history of his specialty, and many specialists have penetrated so deep into the subject that they cease to be amateurs and make most valuable contributions.

Besides, the amateur should seek advice whenever he feels his limitations. This is why it is so important to have institutes of the history of medicine where the amateur can find the best editions of the medical classics, the new literature and expert advice. In the last few years I have prevented the writing of at least half a dozen books; they would have been disgraceful. On the other hand I have helped a great many colleagues in finishing and improving their papers and books.

What will have to be done is to raise the standards gradually. All new medical subjects were started in an amateurish way and, once recognized, were raised to a higher level. Adequate courses in the medical schools can help a great deal. They get the students

interested in the subject, give them correct views and teach them what they can do and what they cannot. I am sure that none of my students will ever attempt to write a biography of Hippocrates, but I hope that many of them will contribute to the history of medicine.

So far, my dear Sarton, we completely agree but I decidedly disagree with you when you reproach the historian of medicine by saying that "he overreaches himself and tells us or seems to tell us that after all the history of medicine is the best part of the history of science." Who was ever bold and foolish enough to make such a claim? Certainly nobody I know.

You wonder why the history of medicine is more popular than the history of science, why there are more medico-historical institutes than such for the history of science. Confidentially, I can tell you that all the institutes of the history of medicine that I know, and I know them all, are in such financial condition at the present time that they are not to be envied. If the history of medicine is popular today, then it is as a hobby but, apparently, not as a historical and medical discipline. Very little money is required to make a medico-historical institute a valuable medical department. But you have no idea how difficult it is to find that little money. There are institutes all over the world, to be sure, but society has not yet fully recognized their importance and is not yet ready to support them adequately. I suppose that this is partly our own fault. We have not succeeded in demonstrating unmistakably the necessity of our studies. Besides, we live in a world in which society has to spend most of its income on armaments and relief and where very little is left for cultural activities.

It seems to me that at the bottom of your statement there is a fundamental misunderstanding that should be cleared up. You begin your article by saying that "the history of medicine has been studied more systematically and by a larger number of scholars than the history *of any other branch of science*." Later, in order to explain the "relative popularity" of medical history you say that medicine "is also and will always remain an 'art'." You might have called it a service, an institution, a social function.

And this is the point. Medicine is not a *branch of science* and

will never be. If medicine is a science, then it is a social science. The physician's goal is to keep his fellow-men socially adjusted or to readjust them if necessary. In order to do this he has to prevent and to cure diseases and he does it largely, though not exclusively, by applying scientific methods. Yet, it would be fundamentally wrong to call medicine an applied science just as it would be a mistake to call geography or technology applied science. A machine can be perfect from the scientific point of view and yet be worthless if it is too expensive to run. Hence, there are other than scientific considerations in technology, too. Medicine, like geography and technology, has an autonomous position in our system of learning.

Now, as the physician has to apply scientific methods, the historian of medicine will have to study the history of these methods. This is obvious and this is why we have so many subjects in common. Nobody will deny that Moritz Roth's *Andreas Vesalius* or the Paracelsus edition of Sudhoff are contributions equally important to the history of medicine and to the history of science. But nobody would be rash enough to conclude that this makes the history of medicine essentially a history of science.

Science is one aspect of medicine and there are a great many other aspects the history of which has to be investigated. The history of medicine in a very large sense is the history of the relationships between physician and patient, between the medical profession in the largest sense of the word (including administrators, public health officers, scientists, nurses, priests, quacks, etc.) and society. We therefore have to study the position of physician and patient in a given society, their attitude towards the human body, the valuation of health and disease at a given time. The scientific standard of a physician alone does not make him efficient. Society has to be ready and willing to accept his advice. The morbidity of a given period, in other words the task with which the physician is confronted, is the result of endless social and economic factors. If you want to understand the development of medicine in the 19th century, you will have to study the industrial revolution first. All these are studies in social history.

The history of hygiene is not only determined by scientific ideas

but just as much by educational ideals, and public health can only develop under favorable political conditions.[1] Medicine, furthermore, is a service that is being sold and purchased. Economic considerations are therefore of paramount importance. The physicians of a given time may be great scientists, admirably equipped for their task, but if society or certain classes of society are unable to purchase their services, medicine will fail in spite of all scientific achievements. Medical history is to a large extent economic history.

One more point. Medicine is not only what the physician does. The majority of all cases of illness—the minor ailments—are never seen by a physician. They are treated by the patient himself or by his relatives. And this family medicine is practised either according to the rules of academic medicine or following the principles of folk medicine in which age old magical beliefs have persisted to our days. Besides, there have always been healing cults. There has always been a minority of the population that sought healing, not in medicine, but in the church with the priests. And here we enter the field of religious history.

Hence, my dear Sarton, when you say that the history of medicine " considered as history of science is essentially incomplete and misleading " you are certainly correct. But you should have added that it is just as incomplete considered as political history, social history, economic history, history of religion and what not. A cat is misleading considered as a dog. And if you say that medical history is " like the play of Hamlet with Hamlet left out," you could just as well have said that Othello is incomplete because there is no Hamlet in it. I am sorry but you are brandishing your pen against windmills, against a conception of medical history that may have existed decades ago but that nobody thinks of sharing today.

Medical history is a complete well-rounded subject considered as what it actually is, namely, the history of medicine. It is essentially a medical discipline and its place, therefore, is in the medical schools and not in the philosophical faculties. Medical history is written by historians who, in addition, are physicians living in close touch with the problems of modern medicine. You remember Bene-

[1] H. E. Sigerist, The Philosophy of Hygiene, *Bulletin of the Institute of the History of Medicine*, Johns Hopkins University, 1933, vol. I, pp. 323-331.

detto Croce saying that "every true history is contemporary history" because it is a live interest that drives a man to consult the past. This applies to medical history as well. Why should a physician undertake the labour of consulting the past, of recreating it in history if it were not that he felt driven to such a task by medical considerations. History is an instrument of life and medical history is an instrument of medical life. It is a vital part of the theory of medicine and contributes ultimately towards the common goal, the improvement of the people's health.

The fact that medical history is given more attention today than half a century ago is explained by the development of modern medicine. Medicine had to specialize by necessity but the more specialized it became, the more the need was felt for a platform, a watch tower from which medicine could be studied as a whole in its developments and tendencies and in all its social implications.

With you I deeply deplore that the history of science has not yet obtained the institutes necessary for continuous and proficient research. The real scholar will do his work even under the most adverse conditions. You have demonstrated this better than anybody else. Whoever has decided to devote his life to research has given up the idea of leading an easy and comfortable life. Institutes, however, are not luxuries but tools, instruments. It is obvious that the men come first. A great artist will play well even on a poor instrument but he will play infinitely better on a good instrument. It is neither rational nor economical to have every researcher in the field start at the bottom, as most of us did, fighting the same initial difficulties, the lack of material available and, first of all, the lack of time, having to make a living in another field. We all have the ambition to see our work continued. We would like to pass on our methods and our experience, to give young people a start. Nothing is more heartbreaking to me than to be obliged to discourage young people from entering medical history, as I so often have to, highly qualified young people willing to work day and night on a starvation salary, yet to discourage them because the little money required is not available or because, as I experienced recently, the previous career was unusual so that it did not fit into the paragraphs of a fellowship programme. Nothing but insti-

tutes can guarantee the continuity of research and they ought to be so endowed as to be independent from temporary grants which often do more harm than good.

I regret that the history of science has only a few institutes so far and I wonder what the causes are. I do not think that it is a matter of popularity or unpopularity. Popularity does not benefit scholarship. On the contrary it kills it. I think that the history of mathematics has produced such remarkable work just because of its unpopularity. Popularity spoils the character of a scholar sooner or later, and the quality of his work suffers by necessity. We all know the type of the scientific salesman, certainly one of the most despicable figures in the world of learning.

There can be no doubt that the history of science today is a most timely subject. It is beginning to be recognized as such. Quite a few symptoms make this evident. Three new periodicals have been founded within a year, *Thales* in France, *Annals of Science* in England, *Philosophy of Science* in America. Most universities endeavor to have some kind of course given and the students are very responsive. Why, then, are not more institutes established?

One, although purely superficial, reason is that an institute of the history of science is a rather expensive affair. If it is to cover the whole field, it must have a large and highly specialized staff. It is impossible for one single man to be an expert in the history of botany and in the history of physics. In medicine, in spite of specialization, we still understand each other, at least more or less. The medical curriculum leads the student through all the specialities and we all serve one common purpose. Science is no longer la science de la nature that could be embraced by one man. It has become still more specialized than medicine. A zoologist, to be sure, must have some chemical knowledge. However, a zoologist and a chemical engineer today have little in common. I know what you are going to say, namely, that the zoologist and the chemical engineer have their history, their general principles, their philosophy in common, that the more specialization there is, the more urgent the need becomes for a synthesis as offered by the history of science. And I certainly agree with you.

I admit that the financial question is secondary. Wo ein Wille

ist, ist ein Weg, and infinitely more expensive institutions are being supported and created all the time. The cause lies deeper. Colbert created the French academies. What made the University of Göttingen a world center of research in the 18th century was the vision and wisdom of one man, the baron Münchhausen who had the power and courage to create new institutions and to call the best men available even if they were not of the front-page-scientist type but just promising youths. One man, Friedrich Althoff, gave the Prussian universities their leading position towards the end of the 19th century, and if Berlin today has a well-equipped institute of the History of Medicine and Science, it is due to the wisdom of the former Prussian minister of education, C. H. Becker.

It is always the same story that repeats itself endlessly. Science progresses, new men open up new horizons struggling against difficulties, fighting prejudices, starving at times but carried on by their enthusiasm—until the statesman enters the scene clearing the ways for further research. It is not our job to beg for funds; we are poor administrators. Our place is in the study, the library, the laboratory, not in the anterooms of government offices or foundations. It is the statesman's duty to create the conditions for research. He has the power and, if he is an enlightened statesman, he must have the vision. If a country claims to be civilized, it must have a definite educational policy, a " Kulturpolitik," as the Germans call it. This is one of the primary functions of government for it prepares the future of the nation. I regret that the word politics has such a bitter after-taste in America but politics in the real sense of the word are everything in the life of a nation. Whether medicine succeeds or fails, whether science and art flourish or starve is ultimately the result of politics.

If the history of science in spite of its importance and timeliness, has not found any adequate support yet, it is due to the anarchic conditions in our present " Kulturpolitik."

European society is decaying. All efforts tend to preserve at any cost a system that has out-lived itself. All energy is concentrated on preparing a war that is accepted as inevitable with a fatalism worthy of Mohammedan nations. You would look in vain for a great figure directing the cultural policy of any European nation.

On a d'autres chats à fouetter. Research is going on, more or less handicapped, following traditional lines, but there is no room for new departures and there will not be before great political events change the structure of European society radically.

The trouble in America is that the man who has the vision and the man who has the power are, as a rule, different individuals. There are splendid university presidents, men of great wisdom, but they have no actual power, they depend on more or less competent boards of trustees, on foundations, on alumni associations, and what not. And besides, their influence hardly ever reaches beyond their own university. The federal government has no cultural policy of its own. The states are primarily interested in elementary education. Some have excellent universities but their budgets are limited, are minor items in the state budget. Yet, we in medicine know better than anybody else what it means to have a real statesman in power. American medicine would not be where it is—and I certainly would not be writing these lines here in America—had not a man of exceptional vision been for many years at the head of the General Education Board.

You will be interested to hear that there is a country where the history of science is given increasing attention, the Soviet Union. I have just spent several months there and I am filled with admiration for what I found. The classics of science, the works of Harvey, Galileo, Volta, Helmholtz, and others were translated into Russian, were printed in editions of 12,000 and 15,000 copies and the last copy was sold within a few weeks after being printed. I tried to buy these editions for our Institute but they were unobtainable, the paper shortage making reprints impossible for the time being. But I found them all over the country in libraries of schools, factories, farms. I met a young mechanic in a collective farm in the South who was studying a treatise of Galvani. He found it difficult, "But," he said, "how can I ever become an engineer, how can I possibly understand modern physics if I have not gone through these texts before?" On a Black Sea steamer I spent an hour near the book stand watching the people, trying to find out what they bought most. And what they did buy most besides political literature were biographies of great scientists of which a whole series was on sale,

biographies of Paracelsus, Virchow, Koch, Pasteur. You, of course, know that the Academy of Science has a strong committee for the history of science and nobody will forget the part played by the Soviet delegation at the International Congress in London in 1931. The Academy is publishing the works of Aristotle and the volumes are issued in editions of 10,000 copies. A translation of Kant's *Prolegomena* was published last year in 15,000 copies, figures that make you giddy, and the last volume of all these books was sold in short time. The All-Union Institute of Experimental Medicine, now in Moscow, is organizing a department of history of science as a matter of course. It did not take the Russians long to recognize what a marvelous instrument they had in the history of science.

There is one last point that I wish to bring up before closing this letter. The history of medicine is not only medicine but obviously also history. It is one aspect of the history of civilization just as the history of science represents one such aspect. Now, you say that "the core of the history of science should not be the history of medicine" (of course not, why should it?) "but the history of mathematics." I have no judgment as far as the history of mathematics is concerned. Being very poor in mathematics I do not understand its history. No doubt that the history of science is an essential part in the history of civilization and yet it seems to me that tradition is not so wrong in assigning the leading rôle to economic and political history. After all, before a man can create cultural values, before he can "approach the understanding of infinity," he must eat. This may be a low function but it is, nevertheless, a necessity. The ways in which man made a living are essential factors in the history of mankind. Economic conditions and politics determine each other constantly and they both create the conditions for cultural activities. If science has a strong foothold in Russia today, if the history of science is cultivated there, this is the result of a new economic order and political regime. And if science is retrogressing in Germany today after so many years of leadership, this is again due to economic and political conditions. I therefore still believe that economic and political history form the core of the history of humanity.

Discussions on the primacy of a discipline, however, are more or less futile. They remind me of the Renaissance treatises *de nobilitate legum et medicinae*. If we are to study the history of mankind, we have to investigate the history of all human activities. All historical disciplines have to co-operate very closely. It is only through joint efforts that we may come to a synthesis.

What I would like to see is the organization of a central institute for historical research in which the history of mankind would be studied in all its aspects by a group of highly qualified specialists working in close contact with each other. The idea is not new. Karl Lamprecht tried to realize it in 1909 when he founded the Institut für Kultur- und Universalgeschichte at the University of Leipzig. Unfortunately, he died six years later and, as the institute was never adequately financed, it never passed the embryonic stage. But even so it was a most stimulating place. I gave several courses there myself in co-operation with the historians of other disciplines and got a great deal out of it. A second attempt was made by Aby Warburg who soon after the war organized the Kulturwissenschaftliche Bibliothek Warburg in Hamburg that has since been transferred to London. The institute has done and is still doing remarkable work. Its starting point was the history of art, its central idea the survival and rebirth of ancient civilization in the Western world. Its field of action is very broad. A Centre de Synthèse Historique was established in Paris by Henri Berr but with entirely inadequate means.

All these institutions were extremely interesting and promising attempts created by men of great vision but they all were established on a small scale and could not possibly embrace the history of mankind. A historical institute is therefore still wanted. It can be objected that the philosophical faculty of any large university is in itself such a group of specialized historians. This is undoubtedly true but the members of any university faculty are burdened with administrative and academic duties with endless routine work and we all know that there is very little actual co-operation between the departments and where such a co-operation exists, it is not systematic but haphazard.

A real historical research institute would have as many depart-

ments as there are basic historical aspects, that is to say, departments for the history of society, economics, politics, law, religion, philosophy, science, language, literature, art, music, technology, medicine. Each department would have a specialized staff. An auxiliary department with archaeologists, palaeographers, bibliographers would serve the whole institute, at the head of which there should be a historian of broad vision able to co-ordinate the work of the departments and to lead it. Great research tasks could be attacked from all angles and joint seminars would help to clarify problems and, at the same time, to train some advanced students. The equipment of such an institute need not be extravagant. No library, of course, would ever be large enough to satisfy its needs, because every book ever written falls into its realm. A reference library and a collection of the essential texts would be required, photographic and other laboratories would supply further material. Such a central institute would be in close touch with all the other historical institutions scattered all over a country. It could serve as a clearing house, as a co-ordinating agency for all efforts in the field.

I have no doubt that such an institute would play a vital part in the life of a nation. It would become its memory and conscience and could not but influence its destiny.

Well, this was a long epistle but your preface got me started and I could not stop before I had emptied my bag. I wish you a happy New Year, yourself, Isis, Osiris ed a tutta la sacra famiglia. Let us not quarrel but co-operate. We are both representing relatively young disciplines, we have much in common and are fighting for the same cause.

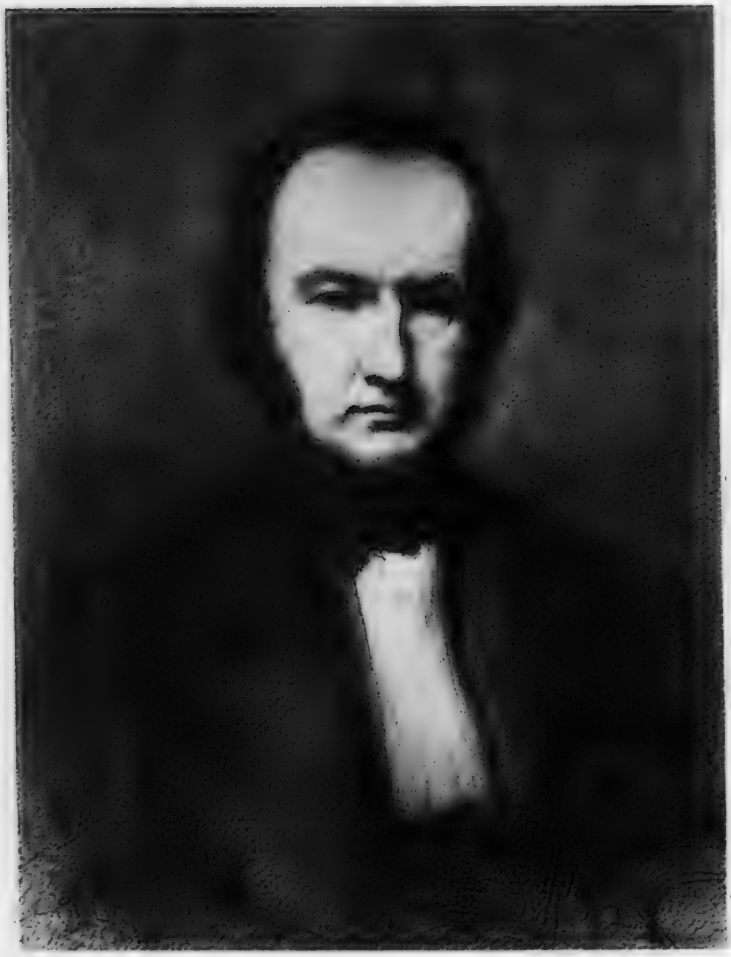
And don't be discouraged. You are doing the work of the pioneer. Someone had to do it. The harvest will follow, some day, somewhere, and the crop will be plentiful. And this is what matters.

In great admiration and devotion, I am

Yours ever,

HENRY E. SIGERIST

Johns Hopkins University,
January 1st, 1936.



CLAUDE BERNARD
1813–1878

THE PHILOSOPHY OF CLAUDE BERNARD

HENRI BERGSON

Address delivered at the centenary celebration of Claude Bernard at the Collège de France, 30 Dec. 1913. Published in "La Pensée et le Mouvant" by Henri Bergson. Paris, Alcan, 1934. Translated by Arnold C. Klebs, Nyon.

The outstanding debt which philosophy owes to Claude Bernard is the theory of the experimental method. Modern science has always adapted itself to experience, but as it began with mechanics and astronomy, as it considered in matter at first what was in it most general and closest to mathematics, it asked for long from experience nothing but to provide a starting point for its calculations and their final verifications. From the 19th century date the laboratory sciences, those that pursue experience in all its nooks and corners without ever losing contact with it. Claude Bernard brought the formula of their method to these more concrete sciences, as formerly Descartes had done for the abstract sciences of matter. In this sense, the *Introduction à la médecine expérimentale* is for us somehow what was, for the 17th and 18th centuries, the *Discours de la méthode*. In the one as in the other case we are before a man of genius who, after first making great discoveries, proceeded to ask himself how one should go about to make them, a course seemingly paradoxical and nevertheless the only natural one, the inverse course, though tried much oftener, having never succeeded. Only twice in the history of modern science and this for the two principal forms which our knowledge of nature has taken, the inventive spirit has reflected upon itself in order to determine the general conditions of scientific discovery. Thus twice, and in France, was brought about this happy mixture of spontaneity and reflection, of science and of philosophy.

The constant thought of Claude Bernard, in his *Introduction,* has been to show us how the fact and the idea collaborate in experimental research. The fact, more or less clearly seen, suggests the idea of an explication. This idea the searcher asks experience to confirm, but all the time while his experience lasts he must be ready to abandon his hypothesis or to re-model it in accordance with the facts. Thus scientific research is a dialogue between mind and nature. Nature awakens our curiosity, we ask it questions, its re-

sponses give to the discourse an unforeseen turn, provoking new questions to which nature replies by suggesting new ideas, and so on indefinitely. When Claude Bernard describes this method, when he gives examples of it, when he recalls the applications he has made with it, all he says sounds to us so simple and so natural that it seemed hardly necessary to say it; indeed we believe to have always known it. In such a way the portrait painted by a great master can give us the illusion that we had known the model.

Still it is necessary even to-day that the method of Claude Bernard should always be understood and practised in its proper sense. Fifty years have passed by his work, we have never ceased to read and admire it. Have we drawn from it fully all that it implies?

One of the clearest results of this analysis ought to be that we learn that there is no difference between a well made observation and a well founded generalisation. We think still too frequently that experience is destined to bring us ready-made facts. Intelligence, appropriating these facts and approaching them to each other, would thus lift itself to higher and higher laws. Generalizing would hence be one function, observing another one. Nothing is falser than this concept of the work of synthesis, nothing more dangerous for both science and philosophy. It has lead to the belief that there was a scientific interest in the assembling of facts for nothing, for fun, to note them down lazily and even passively while waiting for the arrival of a mind capable of mastering them and submitting them to laws. As if a scientific observation were not always the response to a question, precise or confuse! As if observations noted passively one after the other were anything else but answers unstitched from haphazard questions! As if the task of generalisation consisted in the effort of finding after the event, a plausible sense for such incoherent discourse! Truth is that the discourse must make sense at once, otherwise it would have none ever. Its significance might change in proportion to a deeper penetration of the facts, but it must begin by having a significance. Generalising is not utilising, for a so-called condensation, facts already collected, already noted; synthesis is quite another matter. It is less a special operation than a certain intensification of thought, the capacity to penetrate to the inside of a fact which one suspects to be significant and where one would find the explication for an

indefinite number of facts. In brief the synthetical mind is nothing but the analytic mind grown stronger and deeper.

Such a conception of the work of scientific research diminishes singularly the distance between the master and the pupil. It no more allows us to distinguish two categories of searchers, one of which would be common workmen while the others would have the mission to invent. Invention must enter everywhere, into the most humble searching for facts, into the simplest experience. Where there is no personal or even original effort, there is not even a beginning of science. This is the great didactic maxim which resolves from Claude Bernard's life work.

The philosopher sees still something else in it: a certain conception of truth, and consequently a philosophy.

When I speak of the philosophy of Claude Bernard, I do not mean to allude to that metaphysic of life which some have thought to find in his writing and which perhaps was rather remote from his thoughts. Much indeed it has been discussed. Some, referring to passages where Claude Bernard criticises the hypothesis of a "vital principle," asserted that he saw nothing else in life but a collection of physical and chemical phenomena. Others, referring to that "organizing and creative idea" which according to the author, presides over vital phenomena, made him distinguish fundamentally living from raw matter, attributing thus to life an independent cause. According to still others, Claude Bernard wavered between the two conceptions, or rather that he started from the first to arrive progressively at the second. Re-reading attentively the work of the master, I believe you will not find in it either this affirmation, or this denial, or this contradiction. Surely, Claude Bernard rose frequently against the hypothesis of a "vital principle," but wherever he does so, he turns explicitly against the superficial vitalism of the physicians and the physiologists who proclaimed the existence, in the living being, of a force which was capable of fighting against the physical forces and to hinder their action. That was in the time when it was thought currently that the same cause acting under the same conditions on the same living being, did not always produce the same effect. One had to take into account, it was said, the capricious character of life. Magendie

himself who had contributed so much to make physiology a science, still believed in a certain indeterminacy of the vital phenomena. To all those with such views Claude Bernard replies that the physiological facts are ruled by an inflexible determinism, as rigorous as the one of physical and chemical facts. And more, whatever processes take place in the animal machine there is not one which must not be explained one day by physics and chemistry.

This much for the vital principle. But as we pass on to the organising and creative idea, we shall find that wherever it is discussed, Claude Bernard turns against those who refuse to see a special science in physiology, distinct from physics and chemistry. The qualities, or rather the mental dispositions which make the physiologist, according to him, are not the same as those which make the chemist or physicist. He is not a physiologist who has not the sense of organisation, that is of that special coordination of the parts to the whole which is characteristic of the vital phenomenon. In a living being, things happen as if a certain " idea " intervened, accounting for the order in which the elements group themselves. This idea, however, is not a force but simply a principle of explanation. If it did work effectively, if it could in whatever manner counteract the play of physical and chemical forces, there would be an end to experimental physiology. The physiologist must not only take into consideration this organising idea in his studies of phenomena of life, he must also remember according to Claude Bernard, that the facts which occupy him, have for their field an organism that is already constructed and that the construction of this organism, or as he says, the " creation " is an operation of an entirely different kind. Now the neat distinction established by Claude Bernard between the construction of the machine and its destruction or its wear, between the machine and what happens inside of it, might certainly have led him to reinstate in some other form the very vitalism which he fought; but he did not do it, he preferred not to express himself on the nature of life, not any more however than he expressed himself on the constitution of matter; thus he leaves open the question of the relation of the one to the other. Summing it all up, let us say that when he attacks the hypothesis of the " vital principle " or when he appeals to the " directive idea," in either case

his exclusive solicitude is to determine the conditions of experimental physiology. He seeks less to define life than the science of life. He defends physiology either against those who believe that the physiological fact is too elusive to serve for experimentation or against those who, although believing it accessible to our experiences, find these experiences indistinguishable from those of physics and chemistry. To the first he replies that physiological fact is ruled by an absolute determinism and that consequently physiology is a precise science; to the others that physiology has its own laws and its own methods, distinct from those of physics and chemistry, and that consequently physiology is an independent science.

But if Claude Bernard has not given us and did not want to give us a metaphysic of life, the totality of his work presents a certain general philosophy, the influence of which will probably be more enduring and deeper than could have been that of any particular theory.

Philosophers have, for a long time indeed, considered reality as a systematic whole, as a great building that we could, *de jure,* reconstruct by thought with the help of rationalisation alone (by the sole application of reason) though we were obliged *de facto* to call to our aid observation and experience. Nature would thus be an assembly of laws which interpenetrate each other according to the principles of human logic. And these laws would be, ready-made, inside of things so that the scientific as well as the philosophical task would consist in isolating them by scratching from them one by one the facts that cover them, in the same way as one brings out an Egyptian monument by lifting from it grain by grain the sand of the desert. Claude Bernard's entire work protests against this conception of facts and laws. Long before philosophers had begun to insist on what there is conventional and symbolic in human science, he was aware of, he measured the distance that separated the logic of man from that of nature. If, according to him, we could never be over careful in the verification of a hypothesis, we could never be too daring in inventing it. What to our eyes appears absurd is not necessarily so in the view of nature; let us risk the experience, and if the hypothesis is verified, it will necessarily become intelligible and clear in proportion as the facts force us to make ourselves

familiar with it. But we must remember also that no matter how flexible we may make an idea, it will not have the same suppleness as the things. Hence we must be ready to give it up for another which will embrace experience still more tightly. "Our ideas" said Claude Bernard "are nothing but intellectual instruments aiding us to penetrate into the phenomena, they must be changed when they have fulfilled their task, just as one changes a dulled scalpel when it has served for some time." And he added: "This too great faith in reasoning (raisonnement) which leads a physiologist to a false simplification of things originates from a lacking sense for the complexity of natural phenomena." And further he said: "When we make a general theory in our sciences, the only thing about which we can be certain is that all these theories are false, absolutely speaking. They are but partial and provisional truths, necessary to us as the steps on which we repose so to ascend in the investigation." He returned to this point when speaking of his own theories: "Later they will be replaced by others representing a more advanced stage of the question, and so on. The theories are the successive grades climbed by science, widening its horizon." But there is nothing more significative than the opening words of one of the last paragraphs of the *Introduction à la médecine expérimentale*: "One of the greatest obstacles one encounters in this broad and free march of human sciences, is the tendency inherent in the various sciences to individualise themselves in systems . . . it is the systems that tend to enslave the human mind . . . one must try to break the shackles of philosophic and scientific systems . . . philosophy and science must not be systematic." Philosophy not to be systematic! This was indeed a paradox at the time Claude Bernard wrote, and when all tended, so as either to justify the existence of philosopher or to forbid it, to identify the philosophical with the systematic spirit. Still it is the truth, and a truth that will penetrate more and more as there will be developed effectively a philosophy that is capable of pursuing concrete reality in all its nooks and corners. Then we shall not have to face a succession of doctrines of which each, either to take or to leave, pretends to embrace the totality of things in simple formulae. We would have a unique philosophy which would build itself up gradually at the side of science, and to which all who

think would bring their stone. We shall not say any longer: "Nature is one and we are going to try to find among the ideas which we already possess the one into which it might be fitted." Rather shall we say: "Nature is what it is, and as our intelligence, being part of nature, is less vast, it is doubtful whether any one of our actual ideas be large enough to hold it. Let us therefore work at the widening of our thought, let us sharpen our wits, let us break, if necessary, our frames, but do not let us pretend to shrink reality to the measure of our ideas when it is they which must be molded, enlarged, to reality. This is what we shall say and what we shall try to do. But as we advance farther and farther on this route which we begin to tread, we ought always to remember that it was Claude Bernard who helped to open it. For this we can never be adequately grateful to him for what he did for us. And for this we have come to salute in him, not only the physiologist of genius who was one of the greatest experimenters of all time, but also the philosopher who has been one of the masters of contemporary thought.

THE HISTORICAL BACKGROUND OF THE MODERN STUDY OF CONSTITUTION *

ANTONIO CIOCCO

" Just as observers of Mankind have from time to time strained their faculties in the endeavor to find in the external lineaments of a man some clue to his mental power and moral habits, so have physicians sought to find in parallel indications the means of predicting his tendencies in reference to disease." These words of Jonathan Hutchinson (28, p. 4) succinctly describe one example of that important manifestation of human mental activity which in its non-medical aspect falls into the definition of Pareto's residue of combinations. It is not only the foundation on which rest many forms of art but is a very important factor in human relations. Each of us is in some degree an expert in judging from the features of those whom we meet their mental and moral qualities. However good or poor such judgments may in fact be, we all permit them to govern our actions to no insignificant extent. We may not be as ruthless as Franciscus Vallesius, physician to the Spanish King, Philip II, who, according to Lavater (30, p. 8) remarks: " si duo homines inciderunt in criminis suspicionem, is primus torqueatur, qui sit aspectu deformior." Yet, some will agree with the words of the Bible: " a naughty person, a wicked man, walketh with a froward mouth. He winketh with his eyes, he speaketh with his feet, he teacheth with his fingers." (Proverbs: VI, 12, 13). Or with the teachings of Mencius (35), celebrated disciple of Confucius: " Of all the parts of a man's body there is none more excellent than the pupil of his eye. The pupil cannot be used to hide a man's wickedness. If within the breast all be correct, the pupil is bright; if within the breast all be not correct, the pupil is dull." (Le Low, pt. 1, ch. xv.). Numerous examples of similar ideas can be found in all forms of classical and modern art, from the poetry of Homer to the most recent movie characterization.

A rational discussion and justification of these popular and uni-

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versal concepts has been attempted by savants of all periods. Aristotle (1)[1] in *Physiognomica* (Chap. 1) says: "Mental character is not independent of and unaffected by bodily processes, but is conditioned by the state of the body; and contrariwise the body is sympathetically influenced by affection of the soul." Further on one reads: "There never was an animal with the form of one kind and the mental character of another: the soul and body appropriate to mankind always go together, and this shows that a specific body involves a specific mental character. Moreover, experts on the lower animals are always able to judge of character by bodily form: it is thus that a horseman chooses his horse or a sportsman his dogs." The mental and moral qualities associated with different physical characteristics are described in some detail and accompanied by interesting remarks on the methodology of observation. From the supposed behaviour of animals Aristotle draws inferences which he applies to man, as for example (ch. 6): "A small forehead means stupidity as in swine; too large a forehead, lethargy, as in cattle." Similar to this work, both in style and content are those of Polemonis, Greek writer of the third century A. D., and Adamantius, Jewish physician of the fourth century A. D.[2]

The relationship between physiognomy and personality received extensive and systematic treatment by Giambattista della Porta (1536-1615). This Neapolitan philosopher was a typical product of the age that in Italy was to mark the beginning of artistic decline and of scientific revival. He discovered the properties of the "camera obscura" and wrote profoundly on optics and plant ecology; wrote comedies for the theatre and founded an academy to investigate magic. In *Physiognomonia* (18) he quotes freely from the works of Aristotle, Polemonis and Adamantius, cites observations by Plutarch, Lactantius, and other classical writers, and reports in detail the many facial characteristics which accompany different moral and intellectual traits. The idea enunciated by Aristotle on the resemblance between men with certain qualities

[1] The translators remark in the preface: "This work as we have it, is evidently a compilation of two treatises, . . . Neither is Aristotle's, both may be Peripatetic.

[2] Compare the Latin compilation by R. Foerster: Scriptores Physiognomonici Graeci et Latini, B. G. Teubner, *Leipzig*, 1893, in which the Greek and Arabic versions of the above and other authors are also translated and annotated in Latin.

and the animals generally regarded as the prototypes of the specific quality is further elaborated by della Porta and rendered more effective by the striking illustrations in his book. These caricatures of human features side by side with those of different animals are in a way unique and could serve as a satire on *Homo sapiens*. Della Porta's work has remained a classic; but in it, as in other treatises that followed, there is about ninety-nine percent speculation and one percent observation. A good example is given by the book of J. G. Lavater (30) written between 1775 and 1778. The author, a Swiss Protestant minister, looks at all things with the eyes of the man of the Church. What is beautiful and harmonious is holy and good. It is a book unintentionally humorous, due to the author's efforts to show that all men with a good historical reputation had in full the physical characteristics usually associated with virtue, valor, etc.

In more recent times the attempt to correlate intellectual capacity, talents and disposition with physical aspect gave the start to that now discredited branch of constitutional somatology called Phrenology, and to the more respectable subject of Criminal Anthropology. The first is due to F. J. Gall (1757-1828) and J. C. Spurzheim (1776-1832) (47), both regarded as good anatomists for their period. From the fact that diversities in the form of the skull are related to those of the brain they inferred that differential development of the brain could be determined from the shape of the skull. Observations, which apparently lacked precision, on individuals gifted with different intellectual and spiritual qualities (the terms as usual indefinite) led them to a cephalic localization of the different qualities and obviously suggested the possibility of determining from the shape of the skull the presence or absence of certain mental traits. The ease with which quacks availed themselves of these ideas and the very poor quality of their evidence did much to discredit their work forever.

From an entirely different standpoint arose the concepts of Cesare Lombroso (33) which are the bases of criminal anthropology. This author having found certain anatomical " abnormalities " or " stigmata " in criminals—" abnormalities " which are " normally " found in lower animals—was led to conclude that criminality in certain

of its forms is an atavistic trait and that criminals bearing these stigmata are a throwback in the evolutionary scale. The objections leveled at this ingenious theory are grave and derive from the careful studies of Goring (25), Perrier (40) and others. They are: (a) that the incidence of some of the stigmata is apparently as frequent in non-criminals of the same social class as in the criminals; (b) that the alleged somatic differences between criminals and non-criminals are not evident when measurements are taken and statistically analyzed. Lombroso and his students answer by affirming that some non-criminals are potential criminals but do not actually become such because of cultural and environmental conditions. Moreover they justly observe that the comparative data used by their critics are also defective for other reasons than the preceding. Further study is here certainly necessary.

Parallel to the development of Physiognomy is that of the theory of temperaments. Somatological variation and its relation to health and disease had not escaped the notice of physicians. Daily observations taught them that morbid conditions are selective in their incidence and mode of action. Thus Hippocrates (27): "Persons who are naturally very fat are apt to die earlier than those who are slender," (Aphorism 44) and later "Largeness of person in youth is noble and not unbecoming; but in old age it is inconvenient, and worse than a smaller structure" (Aphorism 54). Both of these observations have been precisely confirmed by the statistical studies of life insurance companies. The *Airs, Waters and Places*, which justly deserves to be so frequently quoted, is an essay on constitution in the widest significance given to this word, treating as it does of the relative interplay between individual and environment. In the *Epidemics* (Book I, 3rd Constitution) he describes a fever, from which "they who died were principally infants, young persons, adults having smooth bodies, white skins, straight and black hair, dark eyes, . . .". And again in Book III, 2nd Constitution, of the same work there is a summarized definition of the lymphatic temperament and a statement that individuals possessing this temperament are susceptible to phthisis.

From Hippocrates, Galen and other physicians of the Graeco-Roman civilization has descended to us the fourfold classification

of constitutional types; fourfold because four, according to Empedocles and Pythagoras, were the primary elements from which all substances are constituted: fire, water, earth and air to which correspond the four humours: blood, lymph, black and yellow bile. Health and disease were considered respectively as a state of balance of the four humours. The prevalence of one or the other was the cause of different anatomical, physiological and mental characteristics. Recognition of the patient's temperament was essential in order to establish correct diagnosis, therapy (diet and medicament) and prognosis. Therefore in the textbooks of medicine, throughout the middle ages and up to the modern era, the description of the four temperaments received ample and elaborate discussion. However, nothing was added to what had already been stated by Hippocrates and Galen (24). For example, a contemporary of Harvey, Daniel Sennert (46) (1572-1637) who was professor at Wittenberg and who left among other works an account of the epidemics in the Thirty Years' War, wrote that to study physiology one must first examine the "Naturals without which our bodies cannot subsist whole." These are seven: "elements, temperaments, humours, spirits, parts, faculties, actions" (chap. 3, p. 3). In the preamble to a description of signs that indicate the specific temperament of each organ, of age and sex he states: "Anything is said to be hot, cold, moist or dry" (chap. 4, p. 7). And we are told that heat and moisture give rise to the sanguine temperament, heat and dryness to the choleric, coldness and moisture to the phlegmatic, coldness and dryness to the dry phlegmatic. The same can be found in the treatise of L. Riverius (44) (1589-1655) who taught at Montpellier. This textbook received numerous editions probably because it is an excellent compilation of the best earlier medical works. In brief the principal characteristics of the four types as stated by the above mentioned authors are as follows:

1. *Phlegmatic* (*lymphatic*) *type.* The flaccid obese individual with scarce pilosity and narrow blood vessels; slow in movement and intelligence, even tempered, not much given to gastric or venereal pleasures.

2. *Plethoric* (*sanguine*) *type.* Also obese individuals but robust,

active, moderately hypersexed, with strong and full pulse, and good appetites.

3. *Bilious type.* Thin, dry, energetic, intelligent folk, fastidious of food but with the strongest inclination to Venus; with strong and rapid pulse and good blood vessels.

4. *Melancholic type.* Thin, dark individuals, with hypertrichosis, narrow blood vessels, slow pulse, great appetites, but constipated; not inclined to much sexual activity.

With the progress of science the humoral theory was discarded, (or perhaps we should rather say went out of fashion because there are now signs of its revival in altered form) but the types just described have always remained, with only slight modifications at the background of medical thinking about human constitution. Thomas Laycock (31) professor at Edinburgh in the early part of the nineteenth century gives the following classification: " 1. Persons nervously active from predominent innervation are of the *nervous temperament.* 2. Predominant sanguification and activity of the vascular and muscular system mark the *sanguine temperament.* 3. When there is a predominant innervation and muscular activity with predominant carbon deposit, or excretion, it is the *fibrous* or *bilious temperament.* 4. When muscular activity is good without predominant innervation or sanguification, but with a tendency to fat-formation or deposit, it is the *phlegmatic temperament.* 5. Innervation, sanguification, and muscular activity are defective, comparatively, in the *lymphatic temperament.* 6. In the *melancholy temperament,* with a tendency to carbon deposit, there is comparatively defective innervation, sanguification, and vascular activity."

The above descriptions show plainly, as has been stated, that the concept of health was associated with a balanced humoral constitution and a well-proportioned somatic development. In fact, Riverius (loc. cit. Lib. II, p. 26) says: " sanitas symetria quaedam est, morbus ametria." However, no definition of what constitutes a well developed body is found in any of the works cited. The only quantitative data on body proportions available until modern times were (with one exception) those incorporated in the writings of painters and sculptors. These artistic canons on body proportions have been

critically examined by Adolphe Quetelet in his *Anthropométrie* (41) (Book 2). He notes that the measurements made on Egyptian and Greek statues give evidence that the artists adhered very closely to fixed rules in determining the relative proportions of body segments. *Symetria* as the Greeks called the study of body proportions was apparently first taught by Parrhasius who lived in the Golden Age of Pericles. However, the oldest written canon to have survived is that by the Roman architect Vitruvius who was in the service of the Emperor Augustus. The moduli employed by him are the cubit, distance from the elbow to the finger-tips, to equal one-fourth of the stature; the foot, to equal one-sixth of the stature; height of head, to equal one-eighth. It is interesting to learn that the proportions enunciated by Vitruvius are remarkably close to those derived by Quetelet from his anthropometric data on the Belgians. The principles of Vitruvius were slightly modified by Leon Battista Alberti, Leonardo da Vinci, Dürer and other celebrated artists of the Renaissance who took pains actually to measure a series of selected individuals. The same was done by John Sigismund Elsholtius (20) in his dissertation for the doctorate from the University of Padua, published in 1654 under the title of *Anthropometria*.[3] This treatise presents the first exposition of an anthropometric technique and the main object of the work is to advocate the use of measurements to differentiate the normal from the pathologic state of the body.[4] It was not until 200 and more years later that this was actually done. That is, not until with Beneke, De Giovanni, and others the study of somatic constitution received its scientific foundation. Until then knowledge of the subject remained more or less at the level reached by Hippocrates and was almost entirely disregarded during the furore created by the great biological discoveries of the past century.

Before proceeding to a more detailed review of the progress in somatological constitution it is well to define with some precision what is considered the main objective of investigations in this field.

[3] Quetelet unaware of this work believed that he himself had coined the word Anthropometry.

[4] The author is grateful to Dr. Raymond Pearl and Miss H. Grimm for permission to avail himself of their English translation of this book which is as yet unpublished.

Pearl and Ciocco (37) have recently pointed out that "the study of human constitution has for one of its major objectives the discovery of stable organic correlations (integrated biological relations) between the morphological, physiological, psychological, and pathological characteristics of the individual, and eventually the precise numerical measurement of such correlations. In particular, with regard to the relation between somatic constitution and disease, it seeks the degree of consilience between variation in morphology and in normal or pathologic physiology; and the part this plays in predisposition and different individual reactions to infections and other diseases." This definition is a compendium of the concepts which underlie the works of De Giovanni (17), Viola (49), Pende (39), Pearl (36) and others, on this subject which received its scientific foundation from the studies of Beneke (9, 10). This pathological anatomist in 1878 and 1881 published data on the measurements of various internal organs and the variations in size associated with age, disease and somatic type. His classification of bodily habitus consists of two main categories. The first morphologic combination or scrofulous-phthisical constitution so-called because of the diseases frequently found associated with it, corresponds to the asthenic type of our present terminology. The individuals so classified are characterized by a slightly developed muscular and skeletal system, enlarged lungs, small liver. Beneke's second morphological combination corresponds to our present-day pyknic type, and was by him found associated with rachitism in children and carcinoma in adults. These individuals tend to adiposity, have strongly developed muscular and bony system, large heart, wide blood vessels, small lungs, and moderately large liver. Beneke's works at the time found an echo only in De Giovanni who from these observations and his own clinical experience was to conclude that the cause of the special morbidity of an organ system resides in its disproportionate development relative to other organs. De Giovanni added more physiological and clinical data to those collected by Beneke and noted that not one but a series of diseases are associated with each body habitus. Two very important contributions were made by him. The first was to observe the changes in morphology, and consequently of morbidity, due to age. In this respect he advanced farther than any since and advocated as a dis-

ease preventive a continuous check of the body growth and a rational correction of incipient defects. The second important contribution is his use of anthropometry in the clinic and his attempt to define more precisely the morphologic types.

The subject of measurements and classification of types is of great importance for the subject of human constitution and one which has not been yet thoroughly systematized. From Pearl and Ciocco's definition quoted above it appears that knowledge of the variations in bodily dimensions and of the correlation between them is fundamental. There is a wealth of anthropological data but much of it, due to the omission of facts unimportant for the observers but essential for constitutional study, is practically useless for this purpose. Of these facts one might mention age, disease incidence, and also good statistical technique. The anthropological contributions most worth considering in this respect are those of Davenport and Love (16) who statistically analyzed with respect to disease and other factors the data taken on army recruits in 1917 and 1918 and demobilized troops in 1919, and of Reed and Love (42, 43) who have studied on army officers the changes in weight and height associated with the appearance of tuberculosis, valvular and degenerative heart lesions. Useful data on the measurements of internal organs and their covariation with external dimensions are scarce, also for the same reasons. Aside from the work of Beneke, there are the publications of Bennett Bean (5), Castaldi (13) and his students, Roessle and Roulet (45), and the very recent studies of Pearl, Gooch and Freeman (38) from this laboratory. The association between the variations in body structure and the physiologic state of the individual has been the object of more careful and numerous investigations. There are however many doubtful points in method that should be given greater attention. It is frequently overlooked or disregarded that the usual classification of bodily habitus into asthenic, intermediate, and pyknic, represents really an arbitrary triple division of *a continuous variable* having at one extreme of the distribution individuals in whom the vertical dimensions are a relative maximum and the horizontal a minimum (asthenics), and at the other those in whom these relations are reversed (pyknics). The usual method of typing by visual inspection results in indeterminate class limits, and as yet no standard has been

proposed to arrive at some constant or universal criterion. To avoid the subjective factor in judgment various indices have been proposed. Those most frequently used include the index of Quetelet-Bouchard (Weight/Stature), of Gould-Kaup (Weight/Stature^2), of Rohrer (Weight/Stature^3), of Brugsch (Chest girth/Stature), of Pignet (Stature — (Weight + Chest girth)), of Pignet-Verwaeck (Weight + Chest girth/Stature), of Pignet-Pelidisi ($\sqrt[3]{10}$ Weight/Sitting height), and of Martinet (Stature/Transverse Chest diameter). None of these is entirely satisfactory, limited as each is by the inadequacy of the measurements used and the form in which it is expressed. Several are quite incapable of rational physical interpretation because of the dimensional heterogeneity they involve. For the purposes of the definition and study of somatic constitution, in the writer's opinion, an index besides being simple, clear and significant should be definitely correlated with (a) the external somatic conformation, and (b) the internal anatomic structure. To satisfy the first condition we must know the degree of relationship between the dimensions used in the formula and those they represent. Thus, the index Weight/Stature actually expresses only a certain amount of weight for each unit of stature. Its use as an index of body build is justified by the fact that weight is highly correlated with the horizontal dimensions and stature with the vertical ones. However, it is evident that outside the limits of the intermediate euplastic habitus this index cannot be very enlightening. Hauchmann (26) who has given serious attention to the value of indices has calculated the degree of correlation between the body build as estimated visually and the results from the application of the several formulae. He finds the highest coefficients from the use of Pignet's index (r[5] $= -0.76$), or of Gould-Kaup's ($r = +0.72$) and of Brugsch's ($r = +0.71$). He concludes that Pignet's index is the most adequate expression of somatic structure and yet we must call attention again to the fact that a rational statement of the precise physical meaning of a particular value of a Pignet index is impossible to give. In this respect it unfortunately resembles many of the index numbers used by economists. Benedetti (8) more recently has reexamined the problem. He compares the re-

[5] Just what type of correlation coefficient has been used is not stated.

sults obtained by the use of Viola's trunk index/limbs index [6] with those of other indices. He arrives at the conclusion that none of the latter is satisfactory. He also points out that Hauchmann's results may be vitiated by spurious correlation.

The second condition stated above is fundamental for investigations of somatic constitution. It is the anatomical relationship and variations that are to be sought out and stressed in the study of the soma. For example, using the chest girth/stature index one may find two individuals with practically the same measurements. In one, though, the chest is normally flat and in the other emphysematous. This diversity is of clinical importance.

To summarize, the state of confusion that exists regarding the use of these indices as synthetic expressions of body type is mostly due to the automatic adoption of the methods of Physical Anthropology without the realization that the objectives of constitutional investigations are different. Another important question which arises in the matter of typing individuals is that of the relative value to be given to the adipose and muscular development which may be different in individuals of the same skeletal habitus. Here again it is necessary to arrive at some criterion which satisfies the second condition given above.

It is to the credit of Viola, a student of De Giovanni, that in many years dedicated to this study he has perceived these problems and attempted to solve them. His main contribution has been the development of the rather complicated index described above which expresses synthetically the value of the trunk and of the extremities relative to the average for the population of a certain region or country. From his index one is able to judge also the degree of dysplasia between trunk and extremities. One may object to the complexity of this index which is unwieldy for statistical purposes but nevertheless the idea is a real step forward in the progress of this branch of study.

Following the lead of Beneke and the others mentioned, Draper (19) has added to the study of somatological constitution by his

[6] Trunk index = (transverse chest diameter x chest depth x sternal length) + (transverse hypochondriac diameter x anterior-posterior hypochondriac diameter x xiphoid-epigastric length) + (biiliac diameter x anterior-posterior hypochondriac diameter x pubic-epigastric length). The limbs index = arm length to wrist + leg length to malleolus.

clinical observations coupled with the use of anthropometric technique. His investigations have led him to describe different profiles or somatic types associated with different diseases. The work however has not yet received general confirmation and, though careful, it suffers from insufficient material and inadequate statistical analysis. A recent investigation by Feigenbaum and Howat (21), modeled upon Draper's ideas and methods gives no evidence to support some of his conclusions.

The existence of variations in somatic structure may be regarded as due, *ceteris paribus*, to the organs that influence growth. Therefore, Pende, an endocrinologist and student of Viola, defines the somatological types in terms of the increased or decreased activity of the endocrine glands affecting development. This general viewpoint is very promising and its universal acceptance will depend on the progress of endocrinology. One significant feature, noted by Barbára (3), is that it restores both the fourfold classification of Hippocratic medicine and in modern terms its humoral basis.

Constitutional somatology as defined and developed by the authors mentioned considers the morphologic, physiologic, pathologic, and psychologic characteristics as the resultant of the continuous interaction between heredity and environment without any attempt to distinguish between the relative importance of these two factors. Greater stress on the environmental factor underlies the work of Sigaud and his students Chaillou and MacAuliffe (14). They note that the points of contact between man and his environment are four: respiratory, digestive, muscular and cerebral. They classify accordingly the individuals whose soma supposedly indicates a greater development of one of these organ systems, which is regarded as a sign of resistance to a particular type of unfavourable environment. The classification, aside from any judgment of its philosophic foundation, is difficult to adopt in practice because it assumes correlations between various parts of the body and especially between the trunk and face which are very often not to be objectively observed.

The so-called genetic school of constitutionalists which after Martius (34) and at present with Bauer (4) and his associates has contributed to our knowledge of hereditary factors in diseases, appears to disregard almost completely the influence of environ-

ment. For these authors constitution means only the hereditary make-up of the individual, the genotype. According to Martius it is this which actually determines the individual reaction to disease. With regard to morphology, Bauer considers only the so-called and ill-defined degenerative signs which are for him constitutional anomalies. In sum all variations in the different "panels" of the individual have one original and immutable cause, heredity. Such philosophy and some of the speculations derived from it lay themselves open to sharp criticism which both Viola and Pende have not overlooked. Recently Pearl (36) has discussed this subject with a breadth of view possible only to the biologist and with stringent logic has emphasized the interplay of the hereditary and environmental factors and its dynamics. He summarizes: "The biological constitution (total personality) of an individual at a given moment depends directly and immediately upon and is determined by, at least, A—The present functional condition (tonus and balance) and state of activity of the endocrine system, the autonomic nervous system, the vascular and vasomotor system, the central nervous system (psychic influence). All of which in turn, depend in part upon B, the physical chemistry of the blood, and the innate degree of perfection (biological worth) of the anatomical structure in general, and of each particular organ, and the age of the individual." All derive from "the inherited kind and quality of biological organization and from the environmental circumstances and forces, past and present." Investigations regarding the pathologic, physiologic and psychic characteristics associated with one or the other somatic habitus have become more numerous in recent years. Stiller (48) in 1907 reported his observations on the physical, clinical and anatomical characteristics of asthenic individuals. It is a very thorough investigation based on some 1500 patients. Noticeable is the fact that with but few exceptions (e. g., the tenth floating rib) the same characteristics have been ascribed to this habitus since Hippocrates. An important influence upon recent and contemporary thought in the constitutional field is due to the psychiatrist Kretschmer (29) in a work first published in 1921. He investigated the physical and hereditary aspects of psychiatric patients and found definite association on the one hand between the pyknic habitus and cyloid type of psychosis and on the other be-

tween the asthenic, intermediate, dysplastic habitus and the schizoid group of psychoses.

Besides the findings of the authors already mentioned, those of Fici (22), Benedetti (6, 7), Freeman (23), Aschner (2), Ciocco (15), Boldrini (11, 12), Lazarowitz (32), etc., lead to the following apparently definite conclusions.

1. The asthenic individual is frequently afflicted with hyperthyroidism, tends to be predisposed to tuberculosis, nervous irritability, dyspepsia, gastric and duodenal ulcers, presbyacusia, and schizophrenia. He appears to possess a decreased endurance to prolonged war fatigues and a lower reproductive ability.

2. The pyknic, on the other hand, often has thyroid deficiency and is prone to cerebral hemorrhage, hypertension, arthritism, syphilitic aortitis and cyclothymia.

3. The intermediate habitus shows a relatively higher incidence of malignancies and chronic streptococcus infections and a tendency to schizophrenia.

4. Dysplasia is often found with epilepsy and various forms of exudative diseases.

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THE EARLY HISTORY OF THE ADRENAL GLANDS *

With Particular Reference to Theories of Function

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Introduction. The adrenal glands were apparently unknown until 1563, when they were described by Bartholomaeus Eustachius [1] (1520-1574), professor in the Collegia della Sapienza in Rome and contributor of numerous other original observations. That he was their discoverer was denied by some of the early writers. According to de Marchettis [2] (1626-1688), the ancient physicians mentioned these glands. Morgagni [3] (1682-1771) contradicted this statement, quoting Hippocrates in particular to demonstrate that his reference to "magnae glandulae," spoken of by de Marchettis, does not indicate knowledge of the existence of the adrenals. Petruccius [4] stated that the adrenals are mentioned in the third chapter of the Book of Leviticus. Certainly there is nothing in the reference to "renunculum" which would convince one that the Biblical author refers to the adrenal glands. These early references are utterly unconvincing. I have been unable to find anything which would lead me to believe that the adrenals were known before the time of Eustachius, and it is almost certainly with justice that all historians accord him credit for their discovery. He himself stated that no one had previously referred to the adrenals in any medical work.**

Eustachius devoted a brief chapter in his treatise on the kidney

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[1] Eustachius, Bartholomaeus. Tractatio de renibus et primo de eorum structura. *In* Opuscula anatomica. Lugd. Bat. 1707. Ch. 6, pp. 15-16.

[2] de Marchettis, Dominicus. Anatomia, etc. Patavii. 1652. Ch. 5, pp. 30-32.

[3] Morgagni, Joannes Baptista. Epistolarum anatomicarum duodeviginti, etc. Venetiis. 1740. Epist. XX, pp. 391-468.

[4] Petruccius. Cited by Heim.

** ". . . nihilominus ne aliquid in hac tractatione praetermissum esse quispiam mihi jure objiciat: consentaneum esse duxi de quibusdam Renum glandulis ab aliis Anatomicis negligenter praetermissis hoc loco scribere."

to the adrenal glands (*De Glandulis quae Renibus incumbent*). He described their general configuration and their situation, but he offered no explanation of their function. They were termed by him *glandulae renibus incumbentes*.* The newly discovered glands were not, however, to wait long before numerous and varied functions were ascribed to them.

Theories of a space-occupying function. To Spigelius [5] (1578-1625), who termed them *capsulae renales*, the adrenals merely served to occupy the space between the kidneys and the diaphragm, and to aid in supporting the stomach. Highmore [6] (1613-1684) tended to agree with this theory and, in addition, suggested that they might serve to absorb humid exudates from the large vessels near by. Naturally such a simple theory did not satisfy many. Bartholinus [7] answered Spigelius in this way: "1. Nature makes, nor does nothing in vain or inconsiderately, much less does she appoint a noble animated Part, only to fill a space, which she might have filled by making the kidney a little bigger. 2. These props would have been too weak by reason of their smallness. Nor should this use belong to the Stomach alone, but to other neighboring Parts."

Jean Riolan [8] (1538-1605) advanced the thesis that the adrenals

* Other names:

Latin	Renes succenturiati	Casserius
	capsulae atrabilariae	C. Bartholinus
	glandulae ad plexum	Thomas Wharton
	capsulae renales	Spigelius
	glandulae renales	De Diemerbroeck
	glandulae majores ad renes	Morgagni
	glandulae suprarenales	Winslow
	capsulae suprarenales	Riolan
German	Nebennieren, Nierendrüsen	
French	Glandes surrenales, reins succenturieux	
English	Adrenal glands, suprarenal glands, Diverticulae urinae (Coxe)	

[5] Spigelius, Adrianus. De humani corporis, etc. Venetiis. 1627. Ch. 15, p. 265.

[6] Highmore, Nathanael. Corporis humani disquisitio anatomica, etc. Hagae-Comitis. 1651. Bk. 1, pt. 3, ch. 4, pp. 82-84.

[7] Bartholinus, Thomas. Bartholinus anatomy; made from the precepts of his father, etc. (Tr. by Culpeper and Cole) London. 1668. Bk. 1, ch. 18, pp. 48-50.

[8] Riolan, Jean. Opera cum [sic] physica tum medica, etc. Francofurti. 1611. P. 526.

were placed in the situation they occupy in order to sustain the group of nerves above the kidney, a theory adhered to by his son and namesake.[9] They felt that the chief need for such a support for the nerve plexus was to prevent it from weighing too heavily upon the emulgent veins.[10] Molinetti [10] refuted this proposition by stating that the adrenals lay above the plexus and that the plexus itself was so firmly adherent to the vertebra that it could be freed from it only with the scalpel. To Riolan is also attributed the theory that the adrenals served no other purpose than to generate kidney fat in the fetus. Bartholinus [7] found this theory entirely unacceptable. He felt that their "Cavity, Veins, Arteries, Humors" indicated that the adrenals had some active rôle in adult life. He also maintained that such a theory demanded that the fetal kidney always be devoid of fat, which he found was not the case, and, in addition, he pointed out that kidney fat was never made of "that wheyish black juyce" such as the adrenals contained and that no one had ever seen an "oylie juyce" in the adrenal cavity.

Theories relating adrenal function to nerves. Thomas Wharton [11] (1610-1673) also associated the nearby nerve plexus with the function of the adrenals. He was impressed with the large size of the plexus and with the large nerve supply of such a small organ. He suggested that the adrenals received from the nerves some substance, apparently useless in the nerves, which they absorbed and passed into the veins where it did serve some useful purpose. He preferred to designate them *glandulae ad plexum.* Francis Glisson [12] (1597-1677) held a similar view. He described a certain vessel coming from the body of the adrenal which emptied into the neighboring vein. He also described a valve in the vein which prohibited return of blood into the adrenal. To him it was obvious the adrenals secreted some material into the vein. This matter it could not receive from the arteries, for it would be ridiculous to assume

[9] Riolan, Jean (fils). Les Oeuvres anatomiques. (Tr. by Constant) Paris. 1628-9. P. 347.

[10] Molinetti, Antonius. Dissertationes anatomico-pathologicae, etc. Venetiis. 1675. Bk. 6, ch. 7, pp. 302-303.

[11] Wharton, Thomas. Adenographia, etc. Londini. 1656. Ch. 14, pp. 77-94.

[12] Glisson, Francis. Anatomia hepatis, etc. Amstelodami. 1657. Pp. 524-528.

that the adrenals should separate a substance from the arterial blood and then secrete it immediately back again into the venous circulation. He reasoned, therefore, that it must come from the nerves, since they were the only other structures communicating with the glands. Just what this substance was he did not know. He did not feel that it was formed of impurities separated in the spleen and borne from them to the glands. He rather felt that the adrenals absorbed from the nerves a certain nutritious juice which could then be passed into the venous circulation as the body needs demanded.

Collins [13] (1618-1710) suggested that "a further use . . . may be to impart a Fermentative Liquor, flowing out of the Termination of the Nerves, by some secret passages (not yet discovered) into the body of the Glands belonging to the kidney, to dispose the Blood in order to the Secretion of the serous and saline parts, from the Vital Liquor, whose Compage may be opened, and watry Particles conveyed into the Roots of the Urinary Ducts, and from thence through the Papillary Caruncles, into the Pelvis and Ureters."

Theories involving a relationship to the secretion of urine. Collins' theory was not the first which linked the adrenals in some fashion with the secretion of urine. Casserius [14] (1545-1616), one of Harvey's teachers at Padua, felt that the adrenals served as an aid in the secretion of urine. He named them, consequently, *renes succenturiati.* Caspar Bauhinus [15] (1560-1624) is said to have taught the same theory. In his *Theatrum Anatomicum*,[16] however, he stated that their use was not known.

Molinetti [10] early proposed the thesis that the adrenals functioned only as diverticulae of the blood, diverting most of the arterial blood away from the kidneys and thereby preventing the secretion of urine in the fetus. He reasoned that nature must provide some means of avoiding urinary secretion in the fetus, since this would be disastrous. He felt that most of the arterial blood was diverted

[13] Collins, Samuel. A system of anatomy, etc. London. 1685. Bk. 1, pt. 3, ch. 21, pp. 472-473.

[14] Casserius, Julius, *Placentinus.* Cited by Coxe.

[15] Bauhinus, Caspar. Cited by Coxe.

[16] Bauhinus, Caspar. Theatrum anatomicum, etc. Francofurti ad Moenum. 1605. P. 156.

from the kidneys, only as much reaching them as they needed, the remainder being shunted back to the heart, whence it passed through the umbilical vein and placenta into the mother's circulation. Reaching the mother's kidneys, the waste material was eliminated, which explained the fact that pregnant women urinate not only more often but also more copiously than do non-pregnant women. This theory received little support during the succeeding century and a half. It is true that Pozzi [17] (1697-1752) eulogized it in his oration upon Molinetti, and several observers, although admitting that the use of the adrenal glands was unknown, did associate their function with some activity in the fetal state. Duverney [18] (1648-1730) felt that they must be of more use in the fetus and in young subjects than in adults. Lieutaud [19] (1703-1780) merely pointed out the fact that the blood supply of the fetal adrenal was much larger than that of the fetal kidney, and that the fetal kidney received very little blood compared with the adult organ, " d'où on pourrit tirer diverses rémarques physiologiques, relatives à la secrétion de l'urine." Portal [20] (1742-1832), also, taking into consideration their larger fetal and smaller postnatal size, stated that one could not prevent himself from regarding the adrenals as fetal organs. Vaughan,[21] admitting that their use was unknown, remarked, " Perhaps these glands answer one use in the foetus and another in the adult, but what these uses are it is uncertain."

With Sir Charles Bell [22] (1775-1842), however, Molinetti's theory was revived. Thomas Mitchell [23] also subscribed to the opinion that they served to lessen the quantity of urine in the fetus.

[17] Pozzi, Guiseppe Ippolito. Orationes duae, etc. Bononiae. 1732.

[18] Duverney, Joseph-Guichard. Oeuvres anatomiques. Paris. 1761. Vol. 2, pp. 265-268.

[19] Lieutaud, Joseph. Anatomie historique et pratique, etc. (Tr. by Portal). Paris. 1777. Vol. 2, art. 13, pp. 270-272.

[20] Portal, Antoine. Cours d'anatomie médicale. Paris. 1803. Vol. 5, pp. 593-596.

[21] Vaughan, Walter. An exposition of the principles of anatomy and physiology . . . containing the Praelectiones anatomicae of Ferdinand Leber, etc. London. 1791. Vol. 2, p. 132.

[22] Bell, John, and Bell, Sir Charles. The anatomy and physiology of the human body, etc. London. 1829. Vol. 3, p. 354.

[23] Mitchell, Thomas D. The functions of the suprarenal glands. Medical Reposit. 1, 241-243 (1813).

He felt in addition that they served a similar purpose during the first year of life. Lehman,[24] on the other hand, maintained that they were " waste-gates to the kidneys, when from any causes the general circulation is increased, and blood is propelled to them with undue severity," just as he felt the spleen to be a waste-gate to the entire body, the gall-bladder to the liver, the thyroid to the larynx and trachea, and the thymus to the lungs.

In a paper appearing in the first volume of the American Journal of the Medical Sciences, John R. Coxe,[25] Professor of Materia Medica and Pharmacy in the University of Pennsylvania, supported and enlarged Molinetti's theory with great vigor. In regard to those few who had previously espoused a similar thesis, Coxe remarked, " Yet so little have they said to substantiate their opinion, that it appears more like a randon thought, than the result of a deliberate and serious reflection." He then proceeded to present his argument in a manner so interesting and so typical of physiological proof by reason rather than by experimentation that I feel justified in including part of it in outline. First he established the fact that the adrenals occurred almost universally in quadrupeds, from which he inferred that " what is so universal, must be of some great and general utility." They had never been found absent in man, and were the same number as the kidneys; therefore their function was probably connected with that of the kidneys. They had no excretory duct and hence were not " glands within the usual acceptation of the term." They had a very large blood supply. The interior of the gland presented a definite cavity and the capsular vein communicated with the inside of the capsule, " like the splenic vein with the cells of the spleen." They were always much larger in the fetal than in the adult state, in which they were very small indeed. Since this was the most striking fact known about the adrenals, it forced him to connect their function with activity in fetal life. It seemed clear to him that the fetus secreted no urine. All of these considerations furnished irrefutable evidence that the adrenals were

[24] Lehman, George F. An inquiry into the use of the capsulae renales. Medical Reposit. 1, 137-139 (1813).

[25] Coxe, John Redman. On the functions of the capsulae renales. Am. J. Med. Sc. 1, 40-49 (1827-1828).

designed to act as diverticulae to divert blood from the kidneys and lessen the secretion of urine in the fetus. After birth, he pointed out, the descent of the diaphragm in respiration impeded their further growth and had the effect of diminishing the flow of blood to them. Consequently, they served no longer as diverticulae and therefore more blood was diverted to the kidneys so that they began the secretion of urine. He felt, further, that the adrenals perhaps continued to serve in their original capacity in adult life whenever unusual situations arose. In support of this he stated that in cases of retention one never found in the bladder the amount of urine calculated to have been secreted during the period of retention. Likewise in cases of total suppression he thought the function of the *diverticulae urinae* must be evoked in explanation. He quoted from the literature a case of suppression for seventeen days with recovery, another for forty days, still another lasting eighteen months, and another of seven years' duration. His concluding prize case was that of a boy " who never made water, but lived in perfect health to his seventeenth year, a constant diarrhea attending." How else, he asked, could one explain such phenomena? And as to the other numerous theories, he said, " All other uses are incongruous and entirely hypothetical, with scarcely a solitary fact to give them the slightest credit."

Theories involving an alteration of blood or production of fermentative substances. A number of writers associated the adrenal function with some essential alteration of the blood. One group felt that its action was particularly concerned with changes in the blood of the renal circulation. I have already referred to the theory of Collins. Sylvius [26] (1614-1672) thought that the adrenals separated some liquor from the blood which was mixed with the blood returning from the kidneys after the secretion of urine, serving to dilute it and prevent coagulation. Boerhaave [27] (1668-1738) also taught that the adrenals corrected the fluidity of the blood coming from the kidney. Deidier [28] held a similar view, maintaining

[26] Sylvius [de le Boë], Franciscus. Disputationum medicarum pars prima, etc. Amstelodami. 1663. P. 74.

[27] Boerhaave, Hermannus. Cited by Coxe.

[28] Deidier, Antonius. Anatomie raisonnée du corps humain. Paris. 1742. Ch. 11, art. 4, pp. 373-374.

that the humor from the adrenal cavity was emptied into the emulgent vein in order to give back to the blood the "serosité" and lymph that it had lost in traversing the kidney," où il a laissé non-seulement de l'urine excrémenteuse, mais encore quantité de limph & de graisse qu'on observe toujours aux environs des reins."

Tauvry [29] (1669-1701) stated that the adrenal glands strained out a liquor which became oily by remaining for a considerable time within their cavity, and "after that, throw it into the Veins; where the Blood, that was stripp'd of its Fluidity by the Secretion of the Serous Parts, recovers its former State, and its Parts that hang'd but loosely together are compacted by this Licquor." He felt that the adrenals performed the same function for the vena caval blood as the omentum did for the portal blood.

Olhasius [30] is said to have maintained that the adrenals "received the thick and terrestrial Excrements of the kidneys, which remain after their Digestion." This he thought was the reason the right gland was usually larger than the left, since the right kidney was ordinarily the larger and must consequently have more excrements than the left.

One of the most widely debated of the early theories is that of Caspar Bartholinus [7, 31] (1585-1629). He described a fluid of an obscure brown color filling a central cavity in the adrenals, and was led to believe that they absorbed the atrabiliary juice from the blood coming from the spleen and the liver, and that from them it was carried through the blood to the kidneys. He consequently designated them *capsulae atrabilariae*. He apparently was unaware of the details of circulation which his opponents later brought up in refutation of his ideas. His son Thomas, however, not only held the same view as his father, but defended it vigorously against all opposition. Stating that "their *use* hath been hitherto unknown," he proceeded, in his revised edition of his father's anatomy,[7] to state: "We may say, that a thick and excrementitious black-cholerick humor, is detained in these Cases, which had not been purged

[29] Tauvry, Daniel. A new rational anatomy, etc. London. 1701. Pp. 109-110.
[30] Olhasius, Joachim. Cited by Bartholinus.
[31] Bartholinus, Caspar. Institutiones anatomicae, etc. (Ed. by T. Bartholinus) Lugd. Bat. 1641. Bk. 1, ch. 18, pp. 110-111.

from the Blood made in the Liver, or Spleen, or both, but especially that blood which we formerly proved to be made in the Spleen; which is here kept and digested, because it could not pass through the narrow waies of the kidneys. Nor let the ascending of an heavy substance trouble us, which ever and anon happens in the Body, by means of the expulsive and attractive Faculty of some Part . . . But no man hath thought of the waies by which the black blood should be discharged into these Capsules or Cases. The Arteries do easily occur to such as hold the Circulation of the blood. For according to the old Opinion, a way is readily found to these Capsules from the Emulgent, or from the Trunk of the Aorta itself, which bringing Nutriment such as it is, do withal unlade the Excrement of the Arterial blood, which was not evacuated formerly. But how it returns out of the Capsulae, how it comes to the kidneys to colour the urine black, is not so easie to shew, for the Veins end in the Emulgents, or in the Cava itself, seldom in the kidneys, and so either they should perpetually keep that excrementitious juyce, which is unlikely, or send it back again to the Cava and the Heart, or they ought, verily, to enter the kidneys directly by the Emulgent Veins, without any hindrance by the contrary motion of the blood going out of the kidney. This contrary motion a thicker and stronger humor can easily overcome, manifold branches also opposing the same, as in Rivers we now and then see waters run contrary to the stream, by the banks, and in the middle, by reasons of some fountains opened." He proceeded to admit that there was even further difficulty, for the vein from the right adrenal often opened directly into the vena cava. "And in such a chance, truly, either the Capsula or Case is not sufficiently purged, whence arises some hidden Disease; or the circular motion must there be neglected, which in the smallest Vessels doth frequently vary; or if it must be Religiously observed, we must here conceive a Reverberation of the Kidneys, for the Blood flowing back out of the kidneys through the Emulgent Veins up to the Cava, because it discharged only Whey and no thicker Juyce in the kidneys, it insinuates itself by the Vein next the Capsula, and coming back out of the Capsula by the little Arteries, with the Emulgent Arteries it goes again to the kidneys, and from thence is purged by urine." The other theories seemed

to him devoid of reason. As mentioned above, he felt that the atrabile lent dark color to the urine. Hence he said that when the urine was black it was because atrabile was collected in the adrenals in too great quantity, "Where also may be often, doubtless, the seat of some morbifick cause, especially of Melancholy."

Petruccius [4] espoused the same cause and is even said to have discovered valves in the capsular veins placed in a manner contrary to those of other veins and in such a way that they favored the passage of atrabiliary juice to the kidneys. He felt that the circulation of blood to the adrenals was reversed, the arteries carrying venous blood and *vice versa*. Molinetti [10] staunchly opposed the atrabiliary theory on anatomical grounds, as did others. Highmore [6] also differed strongly from Bartholinus. De Marchettis [2] placed in opposition the fact that "melancholy humor" was especially abundant in adults, whereas the adrenals were very small in adults and large in fetal life.

Veslingius [32] (1598-1649) agreed to a certain extent with Bartholinus: "What their use is, is not sufficiently found out; 'tis supposed that they help the passage of the serous moisture, and contract a part of the melancholy, which like a Runnet helps to separate the urine from the Blood."

De Diemerbroeck [33] (1609-1674) felt that Veslingius' idea carried with it great probability. Its weak point, he maintained, was that no passage from the adrenals to the kidneys could be demonstrated. He added this suggestion: "But what if we should say, That that same black Juice is prepared out of the Arterious Blood, and obtains a certain fermentative Power, necessary for the Venal Blood, for which reason it flows from them not to other Parts, but endowed with the same Quality flows through the Veins proceeding from the *Capsulae* to the *Vena Cava*: But neither is this any more than a Conjecture."

To Kerckringius [34] (1640-1693) the function of the adrenals

[32] Veslingius, Joannes. The anatomy of the body of man, etc. (Culpeper) London. 1653. Pp. 19-20.

[33] de Diemerbroeck, Isbrand. The anatomy of human bodies, etc. (Tr. by Salmon) London. 1689. Pp. 127-128.

[34] Kerckringius, Theodorus. Spicilegium anatomicum, etc. Amstelodami. 1670. *In* Preface.

was to secrete a juice to color and animate the blood and produce a fermentation in the heart. This fermentation served in some manner to excite the heart to action.

Theories relating adrenal function to the lymphatic system. Morgagni [35] offered the conjecture that there might be filtered out into the adrenal cavities a humor which was passed into the receptaculum chyli and thoracic duct during fetal life, when these ducts receive no chyle from the gastro-intestinal tract, in order to assure their patency. Welschius,[36] Teichmeyer,[37] and Culmus [38] also apparently associated the adrenals with a similar function. Dumas [39] (1765-1813) felt that they functioned in some manner in the nutrition of the fetus. Although no canal communicating between the glands and the intestinal tract had been demonstrated, he felt that perhaps the lympatic vessels furnished such a pathway. "Nous pouvons donc conjecturer, que les glandes surrénales renferment une humeur de même nature que le lait ou le chyle, qu'elles la versent dans la cavité des intestins chez le foetus, et qu'elles fournissent une matière capable d'exercer les forces digestives, d'operer son acroissement et de conserver sa vie."

Theories concerning an adrenal-sex relationship. Marcus Aurelius Severinus (1580-1656) is given credit by a number of French anatomists,[18, 19, 40] for having discovered an excretory duct from the adrenal communicating with the testicle on the same side. Discovery of just such an excretary vessel was announced by Valsalva [41] (1666-1723) in 1719. Abstracting his publication, the Philosophical Transactions said: "The celebrated Anatomist, Signor *Ant. Maria Valsalva*, already known by his noble Treatise *De Aure humana*, has lately made a considerable Discovery. He has found the excre-

[35] Morgagni, Joannes Baptista. Adversaria anatomica. Bononiae. 1706. Adv. anat. 3, animadv. 31, pp. 65-66.

[36] Welschius. Cited by Kelly.

[37] Teichmeyer. Cited by Kelly.

[38] Culmus. Cited by Kelly.

[39] Dumas, Charles-Louis. Principes de physiologie, etc. Paris. [1800] Vol. 9, pp. 620-622.

[40] Dictionnaire des sciences médicales. Vol. 47, p. 412.

[41] Valsalva, Antonius Maria. Gior. d. Literati. Venice. 1719. Abstr., Philos. Trans. 385, 190 (1724).

tory Ducts of the *Glandulae Renales*, or *Renes Succenturiati*, which discharge themselves into the Parts of Generation; that is to say, into the Epididymis in Men, and into the Ovaria in Women. He has read a learned Dissertation relating to this Discovery before the Acadamy of Sciences in Bologna, in which he undertakes to prove, that those *Renes Succenturiati* are to be reckoned among the principal Organs of Generation." Valsalva [42] supported his thesis not only with this new discovery but with philosophical and other anatomical considerations. He cited the Bible to show the close relationship between the kidneys and the genitals. He even devised an experiment to prove his point, removing one testis and the contralateral kidney (and presumably adrenal) from a dog. Since the animal did not copulate he felt that the experiment substantiated his theory.

Not long after Valsalva's alleged discovery, Ranby [43] demonstrated an artery proceeding in the direction of the gonads from the region of the adrenals which he thought might have been mistaken for an excretory duct.

Although aware of the fact that there is no such excretory vessel, Meckel [44, 45] (1714-1774), too, associated the adrenals with sex function. He stated: "Those observers who admit an excretory duct to the genital organs, necessarily connect the renal capsules directly with them. Other facts, particularly their simultaneous, considerable development in several orders of the mammalia had suggested the same idea to us, before we knew that it belonged to another, but we could not describe the mode in which the renal capsules and the genital organs co-operated." As additional evidence that there was some relationship between the two, he cited a case of ossified adrenals in a castrated cat, and enlargement of an adrenal in a man affected with syphilis. He had himself observed the glands to be ususually large in two persons "addicted to venery."

[42] Valsalva, Antonius Maria. Opera, etc. Venetiis. 1741. Dis. Anat. 3, pp. 93-96.

[43] Ranby, John. Philos. Trans. 387, 270 (1725).

[44] Meckel, Joannes Fridericus. Manual of general, descriptive and pathological anatomy. (Tr. by Doane) Phila. 1832. Vol. 3, pp. 392-396.

[45] Meckel, Joannes Fridericus. Abhandlungen aus der menschlichen und vergleichenden Anatomie und Physiologie. Halle. 1806. Pp. 17, 164-192.

Especially important he considered the fact that in anencephalics both the adrenals were small or absent, and the gonads stunted. Others had also reported maldevelopment or absence of the adrenals in anencephalia, a number of these reports being referred to by Meckel and by Schmitz.[46] * It is interesting that although these early workers sustained their argument with proof that was obviously faulty, subsequent work has established fairly definitely an adrenal-gonadal relationship.

Other theories. Senac [47] felt that the adrenals served to secrete meconium in the fetus. Van Helmont [48] thought that they produced a lithontriptic juice which prevented the formation of renal calculi. Although I was unable to find such a statement,[19] Lieutaud is said [40] to have considered that the adrenals secreted an acrid liquor which prevented concretions in the vena cava.

John Goodsir [49] based his theory of function upon broad embryological grounds: "I have therefore been led to consider the suprarenal capsules, the thymus and thyroid, as organs essentially similar in structure, as developments of the remains of the blastoderma, being formed of a continuous portion of that part. Situated along each side of the spine, from the Wolffian bodies to the base of the cranium, the renal capsules being developed in connection with the omphalo-mesenteric vessels, the thymus to the jugular and cardinal veins and ductus cuvieri; and the thyroid to the anastomosing branches of the 1st and 2nd aortic arches, as organs performing functions, whatever these may be, analogous to those of the blastoderma, differing from them only in this, that the blastoderma not only elaborates nourishment for the embryo, but absorbs it also from without, that is from the yolk, whereas the three organs in question only elaborate the matter which has already been absorbed

[46] Schmitz, Adamus. De renum succenturiatorum anatomia, physiologia et pathologia. Bonnae. [1842].

* The atrophy or absence of adrenals in anencephalia has continued to arouse the interest of numerous investigators, being interpreted by some as evidence of adrenal atrophy from failure of pituitary development.

[47] Senac. Cited by Coxe.

[48] Van Helmont. Cited by Coxe.

[49] Goodsir, John. On the supra-renal, thymus, and thyroid bodies. Philos. Tr. 136, 633-641 (1846).

by the other parts, and is now circulating in the vessels of the more perfect individual." Is it possible to see in his theory some foreshadowing of real endocrinological function?

Heim [50] suggested in his thesis that the adrenals might serve by diverting the blood away from the lungs, during fetal life in particular. In support of his idea he maintained, falsely, that the adrenal glands were found in all animals with true lungs and were absent in those not having them, that they were large in the fetus when the lung was not functioning, that they were larger than normal in phthisis and asthma and in curvature of the spine affecting respiration. The other university theses dealing with the adrenals [46, 51, 52, 53] are merely reviews and offer no new contributions.

Anatomy. I have made no effort to trace the early contributions to the anatomy of the adrenal glands. It seems apparent, however, that hardly less confusion prevailed among the early authors in regard to the anatomy of the adrenals than did in regard to their physiology. Morgagni [3] pointed out some of the early differences of opinion. There was disagreement as to even their gross shape and substance. Some described them as being quite similar to the kidneys in figure and substance, whereas others found them entirely different. There was a wide difference in the early descriptions of the arterial and venous blood supply, the nerves which reached them, and the lymphatics. Perhaps the most disputed points, however, were the question of a cavity in the adrenals and of an excretory duct. By far the greater number of the writers considered them to have a definite cavity, although they differed as to the size of this vestibule, and as to whether it was a single or a multilocular cavity. Riolan doubted that a cavity existed, and Meckel later pointed out that the apparent cavity was in all probability due to postmortem softening of the medulla. In the earlier works are to be found most fascinating illustrations of bisected adrenals showing a central cavity. Bartholinus' anatomy contains

[50] Heim, Georg. De renibus succenturiatis. Berolini. 1824.

[51] Leonhardi, Henricus F. F. De glandulis suprarenalibus. Dresdae [1808].

[52] Nagel, Mauritius. De renum succenturiatorum in mammalibus structura penitiore. Berolini. [1834].

[53] Kelly, Augustus. De renibus succenturiatis nonnulla. Berolini. [1837].

such plates. Most of the observers failed to find an excretory duct. Valsalva and Severinus, however, described one communicating with the testicles, and Wharton and Kerckringius felt that one existed which opened into the vena cava. Altogether, the author in the *Dictionnaire des Sciences Médicales* was not unfair when he said, "La plupart des auteurs qui viennent d'être cité ont imaginé une théorie et lorsqu'ils en ont posé les bases, ils ont crée des conduits, des humeurs que jamais anatomiste n'a rencontrés." Despite individual differences in opinion, and the lack of faithful scientific observation, it is only fair to say that, as a whole, these early investigations did lay the foundation for our knowledge of the anatomy and embryology of the adrenal glands.

Discussion. As has been seen, most of the ideas concerning the function of the adrenals, bizarre as they were, were formulated shortly after the discovery of the glands. Relatively little was added during the succeeding century. In spite of the paucity of contributions, interest in the subject remained alive. In 1716 the Académie des Sciences of Bordeaux offered a prize on the subject, "Quel est l'usage des glandes surrénales?" Many essays were submitted but not one was considered worthy of the prize. In 1718 the secretary of the society, who was no other than Montesquieu, then twenty-nine years of age, was elected to present the decision of the society.[54] The author of *L'Esprit des Lois* very ably disposed of the theses submitted, analyzing the situation so clearly that I am certain no contestant could have been disappointed. It is interesting that in this one instance the author's versatile talents were directed to a purely medical problem. Interposed among his analyses of the various essays is a very exquisite eulogy on the human body, written in his characteristic style: "Lorsqu'on étudie le corps humain, et qu'on se rend familières les lois immuables qui s'observent dans ce petit empire; quand on considère ce nombre infini de parties qui travaillent toutes pour le bien common, ces esprits animaux si impérieux et si obéissans, ces mouvemens si soumis et quelquefois

[54] *In* Caillau, Jean-Marie. Notice sur les glandes surrénales. Suivie d'un discours prononcé sur le même sujet, par Montesquieu. Ann. clin. Soc. de méd.-prat. de Montpel. 2 s., 4, 209-223 (1819).

si libres, cette volonté qui commande en reine et qui obéit en esclave, ces périodes si réglées, cette machine si simple dans son action et si compliquée dans ses ressorts, cette réparation continuelle de force et de vie, ce merveilleux de la reproduction et de la génération, toujours de nouveaux secours à de nouveaux besoins: quelles grandes idées de sagesse et d'économie." One wonders that the young author was not carried away by the force of his own words and led to forsake literature for science.

Writing one hundred years later, Caillau [54] conveyed the discouraging thought that in the interim not one really important contribution had been made and that the question of the adrenal function still remained a mystery. Shortly before, in 1777, Lieutaud had consigned the problem to posterity: "Les *capsules atrabilaires* sont au nombre de ces parties dont on laisse à la posterité le soin de découvrir l'usage; c'est un droit qui lui est acquis, & que nous ne lui envions pas."

In the early literature are numerous references to various pathological conditions of the adrenals: hypertrophy, atrophy, suppuration, tuberculosis. These observations did not, however, lead to any suggestions as to the function of the organs. In the seventeenth century De Diemerbroeck [38] had given this warning: "Hence because the Use of the Glandules is so little known, I am persuaded it happens, that they were never taken into due Consideration by any of our Physicians; Whereas we find that many Diseases arise from their being out of Order. And therefore it is to be hop'd that all Practisers, both Physicians and Anatomists, will for the future observe these Parts more diligently, and by frequent Dissections of dead Carkasses inform themselves what Diseases their Disorder and ill Temperature may occasion." In 1824 Heim wrote, "Pathologia physiologiam illustrat; sed huc usque in renibus succen. expectatio fefellit." Only twenty-five years later, Thomas Addison [55] read a paper before the South London Medical Society in which he described in a clinical-pathological study the disease which bears his name. He laid down so clearly the characteristic findings of adrenal

[55] Addison, Thomas. On the constitutional and local effects of disease of the supra-renal capsules. London. 1855.

insufficiency as manifest in man that his paper marks a turning point in the knowledge of adrenal function. And so with him ends the early history of the subject. His work paved the way for the experimental studies soon to be undertaken, and little was to be added to the clinical syndrome which he described so well. Although experimental efforts yielded little for many years, they eventually bore fruit. In recent years a great body of work has done much to explain the phenomena of Addison's disease, to provide a substitution therapy, and to give us a fairly clear conception of at least the basic functions of the adrenal cortex.

It seems fair to say that during the three centuries which elapsed between Eustachius' discovery and Addison's presentation, very little of importance was contributed to our knowledge of the physiology of the adrenals. The theories propounded by the fertile minds of the early writers are fascinating, perhaps more so because of their variety and their absurdity viewed in the light of our present knowledge, but we can hardly say that they were important in directing us along the right lines. Certainly there is nothing among them to suggest the elaboration of a metabolic hormone. No one suspected their necessity for life, and no one thought of any relationship to growth. It would seem fanciful to consider Kerckringius' idea of a fermentation in the heart as in any way connected with the hypotension associated with adrenal insufficiency or with the cardiac arrhythmias which occur. The numerous theories linking the adrenals with the secretion of urine and with an alteration of the blood hardly presaged the rôle of the adrenal in maintaining a normal water balance and the normal chemical composition of the blood. It is true that Goodsir, although admitting no exact knowledge, did place the adrenal with the thyroid and thymus as similar in kind and function. Perhaps one could read into this some clever insight into endocrinological function. It is true also that Valsalva, Meckel, and others deserve considerable credit, for, inexact as was their conception and inaccurate as was their proof, they did suggest some sort of adrenal-gonadal relationship, a relationship now definitely established. The real impetus to modern investigation and the first great contribution to modern knowledge of adrenal physiology came with Thomas Addison. Before him were only unproductive

speculations. His accurate observations and clear insight are directly responsible not only for his own very lucid description of adrenal insufficiency in man, but also for the experimental and clinical efforts of recent workers.

The author wishes to make grateful acknowledgment to Myrtie L. Shumacker for valuable assistance, particularly with the translations from the Latin.

TEXTS AND DOCUMENTS

A 15TH CENTURY TEXT ON THE MEDICINAL VIRTUES OF MADDER (RUBIA TINCTORUM)

HENRY E. SIGERIST

Some time ago I published a fragment from the 15th century manuscript 277 of the University Library in Montpellier.[1] Among the many scientific and medical treatises of this manuscript there is (fol. 118v-119r) a short treatise on the medicinal virtues of madder that is worth being published. The text reads as follows:

In Christi nomine incipit libellus de virtute herbe momordice id est caranze.

Herba momordica que oritur in montibus intra saxa, habet folia ut vitis nisi quod est magis minuta, et habet punctas acuciores vite, et dicta folia sunt superius viridis coloris valde, et inferius quasi albi coloris, et quasi pilosa parum, et dicta herba invenitur prope mare, sed illa de montibus est magis perfecta.

Et hec herba est regina herbarum, que celi, terre, aeris optinet principatum. Eius virtutes nemini dicas, nisi filio tuo, vel socio fideli, has pro maximo secreto tibi aperio. Et virtutes huius sanctissime herbe sunt que sequuntur.

Prima virtus huius sanctissime herbe est quod consolidat omnem plagam.

Secunda virtus est quod si abscinderis auriculas equi albi et etiam nigri, et posueris albam super nigrum, et nigram super album, curat equos tam cito, antequam refrigescant, mediante suco dicte herbe ponantur, in brevi consolidat.

Tercia virtus ac experiencia, sunt qui coligunt herbam in die sancti Johannis baptiste et cum ipsa curant oculorum infirmitatem, et consolidant omnes nervos.

Quarta virtus quod si aliquis acciperet de dicta herba per dies tres, et ieiuno stomaco cum vino bono, et positus esset ad cordam vel aliud tormentum, non sentiret.

[1] A doctor's family in the fifteenth century. *Bulletin of the Institute of the History of Medicine*, The Johns Hopkins University, 1935, vol. III, pp. 159-162.

Quinta virtus est quod si uni debet abscindi manus vel aliud membrum, et acceperit de illa herba cum bono vino, et biberit, senciet minorem penam et similiter si unus debet mori.

Sesta virtus est si quis accipiet cum vino dictam herbam, augmentat vires ad prelium, et ad omne aliud faciendum.

Septima virtus est, sucus dicte herbe positus denti doloroso, mitigat valde dolorem et confortat gingivas valde.

Octava virtus est, si quis portaret super se pulverem istius herbe, non potest maleficiari ab aliquo, et probatum est.

Nona virtus est, si quis ponet de pulvere super capud dormientis faciet eum dormire per duas horas magis quam dormivisset.

Decima virtus est, quod oleum sanctum dicte herbe valet ad omnes plagas et ad omnes dolores.

Undecima virtus, quod valet (119r) ad dolorem matricis ungendo superius cum dicto oleo calido, et sic rectificat eam et confortat ipsam.

Duodecima virtus est, si domina sterilis intret in uno balneo, et postquam erit balneata, et exierit, esicet se bene, et ungat locum matricis de dicto oleo, et subito coeat, cum viro, cito concipiet, et hoc oleum confortat et consolidat matricem, probatum est.

Tercia decima virtus est, quod signum cicatricis ungendo cum dicto oleo, reparat mirabiliter.

Modus faciendi oleum est sic. Rp. unum vas vitreum, et pone intus oleum vetus, et quanto magis senex, melius est, posito oleo, ponas intus de dicta herba quantum capere potest, per modum, quod oleum supercapiat herbam, et teneas eam ad solem bene cohopertam per spacium unius mensis, et misse omni die, superiungendo de oleo quantum minuit, et de supradictis foliis. In capite mensis, supradictum vas recipe et inpleas de novo et operta eum, ita quod non possit aliquid respirare, et postea facias foveam in terram, ubi sol valde possit venire vel in arena, vel in stercore equino, et pone supradictum vas in arena circhum circha sic quod sit bene cohopertum, et sic dimitte per unum annum,

> et postea in capite anni, extrahe foras, et est tante virtutis quante balsamum, et quanto magis manet in terra, tanto efficitur melius.
>
> Explicit.

While the name momordica is unusual [2] and probably a local designation, caranza identifies the herb unmistakably as madder (rubia tinctorum). It is to be found as warentia in the Capitulare de villis of Charlemagne.[3] The Latin word garantia (garancia, warantia) can be traced far back to the early middle ages.[4] Hence, the French word warance, guarence, warenche, finally garance.[5]

Madder was highly valued and frequently used in antiquity, not only as a dye, but also as a medicinal herb. It is mentioned by Theophrastus [6] who calls it Ereuthedanon and describes it as a diuretic, therefore to be used against pains in the hip and sciatica. Dioscorides has a whole chapter on the plant that he calls Erythrodanon.[7] As his was the most authoritative treatise on materia medica in the following centuries, I quote the whole chapter in the old English translation of John Goodyer: [8]

> Eruthrodanum [some call it Erythodanon ye root, some Dracons, some Cinnabaris, ye Romans Rubia passiva, ye Hetruscians Lappa minor, ye Egyptians Sophobi] or Ereuthodanum, but some call it Teuthrion. The root is red, fit for dying. Some of it is wild, some sown, as in Thabana of Galilee, & Rauenna of Italy, & in Caria being sown amongst the olives, as in ye fields. And it is sown profitably because there is made much gain of it. But ye stalks of it are 4 cornered long, rough, like to those of Aparine, but in every respect greater & stronger, having ye leaves by distances at

[2] It is not in Ducange.
[3] Monumenta Germaniae historica, ed. G. H. Pertz, tom. iii (legum tom. i), cap. 43.
[4] See Ducange.
[5] E. Littré, Dictionnaire de la langue française.
[6] Historia plantarum, IX, 13, 6.
[7] Ed. Wellmann, III, 143.
[8] The Greek Herbal of Dioscorides . . . by Robert T. Gunther, Oxford, 1934, pp. 384-386.

> every joint, lying about like stars in a circle; ye seed round, at ye first green, afterward red, after when it is ripe, black. The root thin, long, red, diureticall; whence being drank with melicrate it helps ye Ictericall & ye Ischiaticall & ye Paralyticall: It drives out much & thick urine, & sometimes also blood. But yet they who drink it must be washed every day & the difference of ye voided excrements viewed. And ye stalk being drank with ye leaves doth help ye bitten of venemous beasts, but ye stalk being drank with Oxymel abates ye spleen. Ye root put up as a Pessum, expels ye Embryo, & ye menstrua, & ye secondines. It doth cure also the white Vitiligines, being smeared on with Acetum.

The following medical writers, Pliny (XXIV, 94), Galen (ed. Kühn XI, 878), Aëtius (Venet. 1534; 36, 41), Paulus Aegineta (VII, 3), have hardly added anything to the subject. Pliny (l. c.) refers to a superstitious belief: " morbum regium sanari hoc frutice etiamsi adligatus spectetur tantum."

If we compare our text with the classical tradition, we will not find any connection whatever and we are justified in assuming that the source is to be sought in an entirely different field, namely, in folk-medicine. The introduction advertising the plant as queen of the herbs, recommending not to divulge this secret lore except to a son or loyal friend is quite in the style of such popular treatises. The mention of St. John Baptist shows that the text originated in the Christian world and the quinta virtus in all probability refers to the mutilation of criminals as practised in the middle ages. Quite a few of the prescriptions are of a superstitious character, others are very vague: it heals every wound (1), it is good for all wounds and all pains (10), it increases your forces for the fight and for whatever you do (6). The herb obviously is recommended as a panacea.

There is no doubt that this libellus, this short treatise, was not written for physicians—but as a literary document of mediaeval folk-medicine it deserves some attention.

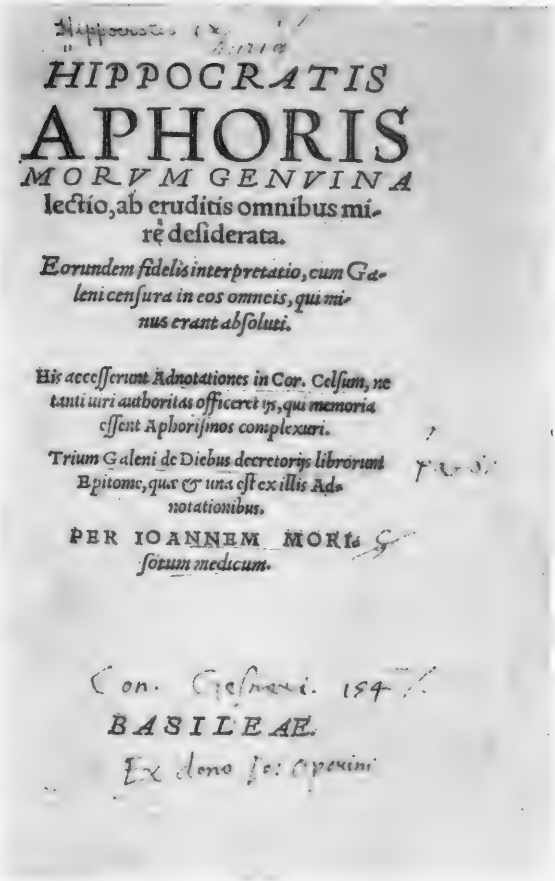
RARE BOOKS IN THE WELCH MEDICAL LIBRARY[1]

1. Conrad Gesner's Copy of the Aphorisms of Hippocrates, Basel, 1547

Among the valuable works of Hippocrates in the Welch Medical Library presented by Dr. Howard A. Kelly, one is of particular interest because it bears the autograph of the distinguished Renaissance physician and botanist, Conrad Gesner, with many marginal notes in his own handwriting. The work is *Hippocratis aphorismorvm genvina lectio . . . per Ioannem Morisotum medicum.* [Colophon] *Basileae, ex officina Ioannis Oporini, Anno Salutis M. D. XLVII, Mense Octobri.* (Fig. 1). Gesner has written on this copy of the book that it was given to him by Oporinus, the famous Basel printer and scholar, who brought out the *Fabrica* of Vesalius in 1543. Gesner's signature has been identified by comparing it with his handwriting as reproduced in Willey Ley's *Konrad Gesner*, Munich, 1929, (Fig. 2). There is proof that the book now in the Welch Library once belonged to Gesner as it is listed in the *Bibliotheca institvta et collecta primvm a Conrado Gesnero, Tigvri, M. D. LXXIIII*, p. 302, sig. C_1^v.

Sanford V. Larkey.

[1] Under this heading we will publish regularly descriptions of some valuable books preserved in the William H. Welch Medical Library. — H. E. S.



HIPPOCRATIS

APHORIS

MORVM GENVINA

lectio, ab eruditis omnibus mirè desiderata.

Eorundem fidelis interpretatio, cum Galeni censura in eos omneis, qui minus erant absoluti.

His accesserunt Adnotationes in Cor. Celsum, ne tanti uiri authoritas officeret iis, qui memoriæ essent Aphorismos complexuri.

Trium Galeni de Diebus decretoriis librorum Epitome, quæ & una est ex illis Adnotationibus.

PER IOANNEM MORIS Sotum medicum.

BASILEAE.

Fig. 1

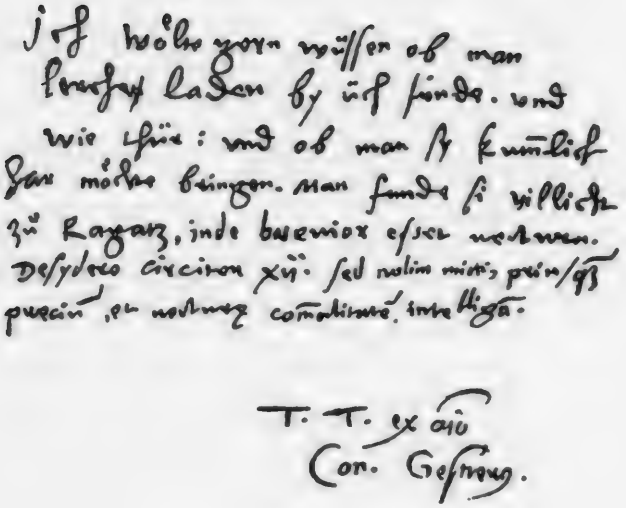


FIG. 2

CONTEMPORARY HISTORY[1]

PREVENTIVE MEDICINE IN EVOLUTION[2]

ALLEN W. FREEMAN, M. D.,
School of Hygiene and Public Health
The Johns Hopkins University

From the title assigned me, and from the annotations made on the notice announcing this meeting, I take it that we are to consider tonight that territory which lies between the field at present occupied by the private practitioner of medicine on the one hand, and the traditional field of the public health authorities on the other. It is in this region that is to be found at once the greatest need and the greatest opportunity of modern medicine. In this almost vacant space between existing services the public health official has been operating for a good many years in a manner highly unsatisfactory to himself and, apparently also, both to the medical profession on the one side, and the public on the other.

In our thinking on this subject we use a number of terms which are not mutually exclusive and which we seldom take the trouble to attempt, even approximately, to define. The terms "private practice of medicine," "hygiene," "preventive medicine," and "public health," among them, comprehend most of the activities with which we are concerned tonight, and it were wise, perhaps, in the beginning of our discussion to attempt to define them as accurately as we may.

By the term "private practice of medicine" we would define the activities of private physicians who in their own offices, or in the homes of their patients, carry on the business of diagnosing and treating disease in individuals who come voluntarily to them for service, and who remunerate them, usually, at a rate dependent in part upon the financial resources of the patient, and in part upon the amount and character of the service rendered.

[1] Under this heading we will publish papers dealing with social aspects of medicine, mostly such papers as have been read before the Social Problems Forum of the School of Medicine.—H. E. S.

[2] An address read before the Students' Social Problems Forum of the Johns Hopkins Medical School, December 5, 1935.

" Hygiene " is capable of reasonably accurate definition. It presupposes the existence of a certain body of knowledge regarding human physiology which can be put into the possession of any individual and which, intelligently utilized, will permit him so to regulate his own life as to promote health and to prevent disease. Innumerable books on the subject have been written and uncounted words have been uttered, in attempting to acquaint the individual citizen with this presumably invaluable body of information.

" Preventive medicine " is a comparative newcomer to the family of words with which we are at the moment concerned. It is, in a very real sense, a modernization of the term " hygiene " as it was first used. We have come to realize, with the advance in medical knowledge, that it is impossible to reduce the technique of promoting health and preventing disease to a simple group of general principles which any individual, anywhere, can successfully apply to his own life. Real hygiene, as we see it today, is a highly individual matter. Those health-promoting and disease-preventing activities which cannot be performed by the individual by the general methods which we will later discuss under the title of public health, cannot in ordinary affairs be applied effectively by an individual to his own life on the basis of things taught him in books or in courses in hygiene. Successfully to adjust an individual to his environment in such a way as to promote health and prevent disease, requires technical knowledge and skill of a high order. Preventive medicine, therefore, is, so to speak, custom-made hygiene.

" Public health," as we shall use the term in this discussion, defines the sum total of those procedures which can be carried on for the protection of the community, without necessarily any cooperation, or even understanding, on the part of the individual citizen. It involves, of course, the sanitation of environment: garbage and refuse disposal, the sanitation of water, of milk and of other foods, sewerage, industrial hygiene, ventilation and lighting. It involves, also, the usual procedures in communicable disease control, such as reporting, epidemiological study, isolation, and hospitalization.

Our terms, therefore, having been defined as well as may be at the moment, let us examine the evolution within the last genera-

tion of the two extremes of the field of medicine, private practice, and public health.

The practice of medicine has, within the memory of your speaker, undergone greater change than had occurred in the whole previous history of medicine. The fundamental cause of this change is, of course, the enormous increase in knowledge in every field of medicine. The result of this increasing knowledge has been the fractionation of the subject into a large and constantly increasing number of specialties, with the consequence that the field of knowledge of any individual physician has tended to become limited to a particular region of the body, or even to a single disease of a particular region.

The physician of the turn of the century, at which time my professional acquaintance with the subject began, was, almost without exception, still a physician, in the sense that even though he might devote a large part of his time to a particular specialty, he believed himself competent to undertake the examination and treatment of any individual coming to him for care. There still remained to him, therefore, the possibility of preserving the historic individual relationship between physician and patient.

In these first thirty-five years of the twentieth century, however, the increasing complication of medical practice, the multiplication of the mechanical equipment required for examination and treatment, and the subdivision of the human body into innumerable regions, each the exclusive property of a separate group of specialists, has destroyed almost completely the individual relationship of the past. General practitioners survive in name, it is true, but they have become in fact only specialists in coughs, colds, and minor illnesses and injuries. The average citizen of Baltimore, able to pay for his medical service, has not a physician, but a group of physicians. From this group he chooses the one most suitable for the particular ailment existing at the moment. This physician has, usually, no knowledge whatever of the other physicians serving his patient at other times for other complaints. Suffering from a serious or obscure illness not requiring hospitalization, a patient may go from one to another of a wide variety of specialists whose services are never properly coordinated, and no one of whom acts as physician to the patient as a whole.

The result of this change in the method of private practice has been a tremendous increase in the cost and in the difficulty of securing proper medical attention. There has been also an almost complete loss of the greatest potential benefit to the patient which results from continuous supervision by a competent physician, familiar with the patient's history, his economic, social, and psychological background, and possessing an integrated knowledge of all of the patient's defects and illnesses.

It must be borne in mind that we are discussing the private practice of medicine as we have earlier defined it. Our patient, if seriously ill, will undoubtedly be sent to a good hospital and will there receive another and, in general, far more valuable sort of service. This other service cannot, however, be considered as part of the private practice of medicine since once in hospital, the patient becomes a patient not of an individual physician but of an organization.

So much for the change in the private practice of medicine: What changes have taken place in public health during this same period?

The health department of an American city at the turn of the century was, by present standards, a simple organization with an extremely limited list of functions. It was concerned primarily with only two general matters: sanitation, and the control of the commoner communicable diseases. In sanitation it conducted a limited program of control of milk and of other foods, the enforcement of a reasonable degree of decency in the matter of garbage and refuse disposal, the enforcement of a fairly rigid system of control of plumbing and drainage, and some measure of supervision of sources of water supply other than the public supply.

In communicable disease, incomplete reports of the prevalence of diphtheria, scarlet fever, measles, mumps, and whooping cough were received. The measures of control were limited, usually, to placarding the premises upon receipt of the report, and disinfection upon notice from the practicing physician that the case had recovered. Smallpox was, when it appeared, usually the object of fairly active measures.

City health officers were at this time almost universally prac-

titioners of medicine, who gave only part of their time to the conduct of the health department. Communicable disease work was performed also by part-time practitioners, and the only other employees of the health department were untrained sanitary, plumbing and food inspectors and the necessary clerks. Deaths were, in most cities, registered with reasonable accuracy, but the reporting of births was everywhere grossly incomplete. With a program and personnel of this character, public health was a thing apart from the practice of medicine. The line between them was easily and clearly drawn.

The first change in public health, the establishment of a diagnostic laboratory as an aid to the more accurate control of communicable diseases, did not significantly affect this relationship. The second change, however, the attempt to control tuberculosis, early introduced new factors into the situation. Tuberculosis was highly prevalent in those classes of the population which did not ordinarily enjoy regularly the services of a private physician. The early diagnosis of the disease was, then as now, beyond the capacity of the general practitioner of medicine, and the prevention of the disease required careful and intensive supervision of sources of infection in the homes over a long period of time. Even the treatment of tuberculosis, dependent, as it is, for success upon early diagnosis, could not effectively be carried out with the facilities then available. To meet this situation two new agencies were created: the tuberculosis clinic, and the public health nurse. The clinic was established to meet an urgent necessity. Unless facilities for early diagnosis were made available, regardless of the ability of the patient to pay, there could be no hope of success in preventing tuberculosis. It was early discovered that even with private patients, able to pay, the practicing physician was seldom willing to take the time and trouble necessary to secure the isolation of the patient and to prevent the infection of the other members of the family group. It was in our own hospital that this fact was first recognized, and the sending by Doctor Osler of a nurse into the homes of tuberculosis patients from the hospital was as influential as any other single factor in making the nurse an essential part of the program of tuberculosis prevention. This occurred in

1903, but it was not until ten years later that the idea was clearly recognized and the name " public health nurse " coined.

Every change which has come into the relationship between the public authorities and the private practitioners of medicine since that time depends inevitably upon the fact that if certain essential procedures in disease prevention are to be made effective in the existing state of private medical practice, it is necessary that supplementary services purely medical in character be organized and conducted by government. If there is to be a battle between the private practitioners of medicine and the public health authorities, it is on this essential principle.

In the thirty years which have elapsed since the inauguration of effective measures against tuberculosis, the program of public health has steadily enlarged, and always on the side of preventive medicine rather than in the direction which we define as the proper program of public health. The prevention of infant mortality and the promotion of infant and child hygiene, have required the establishment of agencies essentially clinical in character. Infant health is promoted by establishing and maintaining continuous medical supervision, beginning as soon after birth as possible. The infant welfare clinic and the infant welfare nurse, are devices for bringing the mother and baby regularly to the physician and for insuring that the physician's instructions are carried out. The success of this program in lowering the mortality of infants and in promoting the physical well being of those who survive, is so evident as to be beyond question. At the present time in Baltimore approximately one-third of all infants born in the city come under the supervision of these agencies, and in the Eastern Health District more than two-thirds of all infants under one are registered and supervised by the infant welfare clinics.

The value of the infant welfare service had not long been established before efforts began to be made to prevent maternal and neonatal mortality, and to improve the hygiene of maternity. The methods to be followed were outlined by the outstanding obstetricians of the country, prominent of whom was Dr. Whitridge Williams of this School. They included, first of all, registration of the pregnant woman as early in pregnancy as possible, medical ex-

amination by a competent obstetrician, and supervision by a public health nurse during the whole of pregnancy. Wherever these methods have been adopted, maternal and neonatal mortality has been lowered and the health of mothers greatly improved.

Along with these movements for improving the health of mothers and young children, there developed slowly but very definitely, a system of medical supervision of the health of school children. The health officers first went into the schools in this country for the purpose solely of better controlling the common communicable diseases of childhood, in the dissemination of which the schools were believed to be an important factor. Visits to schools by physicians from the health department were made in the beginning with this purpose only in view. As is so often the case in dealing with social movements, however, the performance of a minor task revealed definitely a major need. The physician who examined the children in the public schools for scabies or pediculosis could not ignore the great flood of physical defects of all sorts which stared him in the face: defective teeth, enlarged tonsils and adenoids, bone and joint defects, cardiac disease, defects in vision and hearing, and above all, defective nutrition. All were so evident that the school physician became immediately an ardent propagandist for an enlarged and effective system of health supervision of school children. In the face of such a need the remedy could not long escape development, and within a very few years, examination of all children on entrance, and periodic supervision thereafter, became usual and normal parts of the public school system.

Somewhat similar conditions disclosed themselves in colleges and in industry, and it was not long before the value of regular medical supervision became evident wherever large groups of people, particularly of young people, were to be cared for.

Even in adult life there are many who believe that disease may be prevented and the physical vigor of the individual preserved, if he is seen at fairly regular intervals by a competent physician and advised in making those adjustments which advancing age make necessary.

There has grown up amongst us, therefore, for the most part

quite unrecognized, and for the most part, also, under the auspices of official and non-official health agencies, a system which recognizes the principle that once the routine procedures of public health are properly performed, those further additions to the health of the population which our present knowledge makes possible can be secured only by establishing, as soon after birth as possible and maintaining throughout life, a continuing intimate contact between the individual and his medical adviser. Even though not fully recognized in theory by an individual or any group, this idea has been in practice extended to one or another of the problems of public health. It has developed alongside of the ordinary routine practice of curative medicine by the private practitioner. These procedures have been developed by health officials to meet obvious and imperative needs and in the entire absence of any desire to invade the field of the private practitioner or to establish any particular form of medical organization. In almost every phase of this program effort has been made from the beginning to induce private practitioners to render the necessary service. Every health department, burdened as it is by a volume of service vastly greater than it can properly render, would gladly transfer the examination and health supervision of school children to the regular physician of the family from which they come, and innumerable efforts in this direction have been made, not one of which has been even reasonably successful. Every health department would be glad if private physicians would diagnose early tuberculosis in their private patients, but they do not, and apparently can not. If cases of tuberculosis in the great mass of the population are to be discovered, they will be discovered through organized agencies, conducted under public auspices and at public expense.

The control of syphilis is theoretically an entirely possible procedure. The experience in recent years of Denmark and Sweden would indicate that it is an entirely practicable procedure. It depends, apparently, upon two, and only two, factors: the early initiation of treatment and its continuance through the infective stage; and search for, discovery, and effective control of a relatively small number of sources of infection. Experience everywhere has demonstrated that if these things are to be done, they must be done by public agencies. In its effect on the future of our civilization, the

control of syphilis is probably as important as any single question with which we are confronted. Refusal to attempt its solution by the only means known to be of the slightest value, on the ground that such procedures invade the rights and privileges of private practitioners, would of course be absurd.

It is unfortunate from every standpoint that these procedures of preventive medicine, which are in my mind the real medicine of the future, have had to be developed by public agencies rather than by private practitioners. Had these preventive measures been taken up and developed first by private practitioners and the problem of their application to the upper and middle classes been solved by them, the dilemma in which we find ourselves at the moment need never have arisen.

When vaccination was discovered it was taken up by the medical profession like any other medical procedure, and for many years health officers were called upon to vaccinate only the indigent or those especially exposed to smallpox. With the rise of antivaccination sentiment and the disappearance of severe smallpox, however, private physicians almost universally abandoned the practice, and practically all the vaccination done today, for rich and poor alike, is done by the public authorities.

The possibility of the control of diphtheria presented by the discovery of effective immunizing agents would apparently not have been realized in this century if the promotion of the use of these agents had been left to private practitioners.

It would appear that there is a real and fundamental incompatibility between the private practice of medicine as it is organized today, and the general practice of those preventive procedures which seem to offer most for the future of individual and community health. Real preventive medicine requires a continuous relationship between physician and patient. It requires that the physician's contact with his patient be not limited to those brief periods of actual disturbances of health that occur at irregular intervals. It requires that certain services be rendered at stated times: immunizations and examinations of special sorts, should follow a regular and invariable routine. It requires, for a large part of the population, at least, an amount and quality of service which can

not be paid for on the usual fee basis. It probably, for a considerable proportion of the population, must needs be rendered at public expense. It will require, probably, if it is properly to be done, organization and systematization to the end that the service may be rendered efficiently and at reasonable cost.

As you will now have recognized, we have avoided with purpose any discussion of medical treatment. If in our preventive program a certain amount of treatment must be rendered, this treatment must be considered as given for the benefit of the community in which the individual lives rather than for his own. It would seem to us logical to believe that these preventive procedures must inevitably become a part of any system of medical care. Any system which limits itself solely to cure, and which is apparently incapable of rendering these preventive services, will inevitably be superseded.

The fatal obstinacy with which the official organization of the medical profession in this country clings to the idea that the only possible medical service is one which depends upon the service of an individual physician rendered to an individual patient and paid for by an individual fee, may well prove to be the deciding factor in insuring that the medicine of the future will be completely socialized.

MEDICO-HISTORICAL ACTIVITIES IN THE UNITED STATES

DISTRICT OF COLUMBIA

WASHINGTON

On November 21, 1935 Dr. P. M. Ashburn, Colonel, United States Army, retired, formerly Librarian in the Army Medical Library, delivered an address on "The Medical History of the Conquest of the Americas in the 16th and 17th Centuries" at the Cosmos Club before the Washington Academy of Sciences and the Medical Society of the District of Columbia.

LOUISIANA

NEW ORLEANS

The History of Medicine Society of Tulane University of Louisiana was formed last year by a group of enthusiastic medical-history minded undergraduates. The group met weekly in the campus room provided for the Society, and discussed papers which were prepared and read by the members. At first the material presented was arranged chronologically, following Garrison's splendid text as a guide. Later this plan was abandoned, and members reported on subjects which especially interested the group at the particular time, or which interested the particular member preparing the report. Among the topics presented, were:

"Egyptian Medicine," "Sumerian and Oriental Medicine," "Greek Medicine," "The Byzantine Period," "The Mohammedan and Jewish Periods," "The Medieval Period," "The History of Purpera," "Sydenham," "Boerhaave," and "Laennec."

In addition to the papers prepared by the student members, members of the faculty were asked to contribute papers, and among those presented were:

"Anatomical Art"
Prof. Harold Cummins
"Brown-Sequard"
Prof. H. S. Mayerson
"Sir Wm. Osler"
Dr. J. H. Musser, Jr.
"Dr. Dennis"
Prof. Stella Leche
"Otto Folin"
Prof. Sidney Bliss
"History of Tulane University of La., School of Medicine"
Mr. Joe Roberts
"Comments on Medical Schools in the South"
Dr. Roy Turner

"History of Digitalis"
Dr. J. T. Halsey
"History of Our Knowledge of the Function of the Carotid Sinus"
Prof. H. M. Sweeney
"William H. Welch"
Dr. Charles W. Duval

This session the Society is sponsoring a series of lectures given by invited speakers, and open to the medical school student body. The lectures so far announced are:

"History of the Medical School, Tulane University of La."
Dr. C. C. Bass
"Elie Metchnikoff"
Prof. Harold Cummins
"History of American Physiology"
Prof. Henry Laurens
"Lord Lister"
Dr. Alton Ochsner
"Theobald Smith"
Dr. Charles W. Duval
"Claude Bernard"
Dr. H. S. Mayerson
"The History of the Concept of Specific Disease Entities"
Dr. Roy Turner
"John Herr Musser, Sr."
Dr. J. H. Musser, Jr.
"American Women in Medicine"
Dr. E. Bass
"N. O. Medicine in the Post Civil War Period"
Dr. Fossier
"Chinese Medicine"
Dr. E. C. Faust
"Rudolf Matas"
Dr. I. Cohn

B. Bernard Weinstein

MARYLAND

Baltimore

Johns Hopkins University

Institute of the History of Medicine

Lectures

On November 21st Dr. Sigerist delivered a lecture on "The Medical Profession through the Ages" at *Lehigh University*, Bethlehem, Pennsylvania before the Robert W. Hall Pre-Medical Society.

On November 25th Dr. Sigerist delivered two lectures at the *University of Virginia*, one before the third and fourth year classes of medical students on "Medicine in the Soviet Union," and the other before the Medical Society and faculty of the medical school on "Primitive Medicine."

Meetings

The *Johns Hopkins Medical History Club* held its first meeting of the year on December 2, 1935 in the Institute of the History of Medicine. The following four papers were read:

"The Hippocratic Oath in Elizabethan England"
Dr. Sanford V. Larkey

"Galen's Biological Conception of the Soul"
Dr. Owsei Temkin

"An Unpublished Autograph Letter of Dr. John Crawford. (1746-1813)"
Dr. John Rathbone Oliver

"The Tenth International Congress of the History of Medicine in Madrid"
Dr. Henry E. Sigerist

Refreshments were served after the meeting in the Great Hall where several exhibits were displayed showing new acquisitions of the Institute and a collection of books illustrating Dr. Larkey's paper.

Gifts

A splendid collection of Sinhalese manuscripts, books and objects was presented to the Institute by Dr. W. P. Jacocks. The collection includes the following Ola Leaf manuscripts:

Vattoru-Veda-Pota, *A Book of Prescriptions.* 137 Ola Leaves. This manuscript contains valuable recipes and treatment for hydrophobia, snake bites, general diseases, and method of piercing arteries and burning of different parts of the body for curing diseases and also treatment of cattle and elephant diseases. Date and author are not given. Copied in the early part of the 19th century. A valuable manuscript.

Vattoru-Veda-Pota. 179 Ola Leaves. This manuscript contains charms and medicines for diseases made by demon spirits and also symptoms and diagnosis of different diseases and their treatment. The recipes are written in prose and poetry in Sinhalese. Date and author are not given. Copied in the early part of the 19th century.

Sarasankshepa. A work on medicine. It contains stanzas in Sanskrit (in Sinhalese characters) and their paraphrase in Sinhalese by Rajaguru Sri Chandra. This appears to be a compilation from the well-known medical works such as Sushruta, Charaka, etc. with copious quotations from these books. This book deals on the treatment of

various forms of diseases and gives a large number of special recipes. The date is not given. It may have been copied over seventy-five years ago. The book is incomplete.

YOGAPITAKA. 289 Ola Leaves. A Sinhalese prose work on medicine. Date 1888.

A BOOK OF CHARMS. This is a Yantra book of over 68 Yantras. It contains Yantras on Charms for protection of lands, houses, children and fever. For head-ache, for prosperity, for diseases of females, for law-suits, and to ward off sickness. For different Yakshas. These Yantras are copied on olas charmed and tied around the waist, neck and arm. No date is given. Probably written over ninety years ago.

BHESAJJA KALPAYA. 144 Ola Leaves including two extra leaves at the end. This is a medical work written in Sanskrit slokas (in Sinhalese characters). It contains divisions of chapters on seventy-one kinds of diseases such as fevers, piles, etc. and their pathology, premonitory symptoms, different types, prognosis, treatment, etc., and prescriptions for them such as decoctions, pills, pastes, oils, etc. This is a very important medical work used in Ceylon by Vedaralas. It is written in a legible hand by a low country scribe about fifty years ago. There is a separate independent content in Sinhalese at the end of the book.

YOGARATNAKARAYA. *Mine of Precious Prescriptions.* 639 Ola Leaves. A work on medicine in Sinhalese verse written about the end of the 16th century. Its authorship is doubtful: some think that it was compiled by Totagamuwe Sri Rahula Thera the Great Poet. The materials for the compilation of this work seem to have been derived from various medical treatises of India and Ceylon origin. The work treats of the symptoms and treatment of nearly all the diseases known to the Sinhalese at the time of its composion. The book was written probably by a low-country scribe in legible hand in the 19th century, but the characters are not very well formed. The date is not given.

VARAYOGASARAYA. 222 Ola Leaves. This is a Sinhalese work in prose which generally follows the line of the Sanskrit work Sarartha Sangraha. The Varayogasaraya, though a comprehensive one, is less scholarly in style and arrangement than any of the other well known medical works in Ceylon. The name of the author and the date of compilation are unknown.

SARARTHA SANGRAHAYA. 136 Ola Leaves. This is a work on medicine written mainly in Sinhalese, but frequently interspersed with Sanskrit quotations. This is the oldest work on the subject of those written in Ceylon now extant by king Buddhadasa who reigned at Anuradhapura from 341-370 A. D.

PALMYRAH-LEAF MANUSCRIPT. "Chinthamani" or the Tamil Sys-

FIG. 1

FIG. 2

tem of Medicine is traced to the Great Sage "Agasthirar" and the eighteen Tamil Saints who followed him. Those Saints are supposed to have lived about 2,000 years ago. The System of Medicine propounded by them was handed down from generation to generation as an unwritten science which was committed to memory by a long array of medical students and physicians that lived from time to time. In gradual course, this ancient lore assumed a documentary shape in the form of palmyrah-leaf manuscripts. Physicians have been making out newer and newer copies from the originals. Thus all the present day Tamil physicians own copies of palmyrah-leaf manuscripts containing a disquisition upon the system of medicine and also the numerous prescriptions indicated for various diseases. The present manuscript is also such a copy which is approximately seventy years old. The manuscript was under use by a Tamil physician, one Visvanathachari of Nagercoil who passed away about twenty years ago.

The manuscript is only one out of a large assortment the entire study of which alone could give a complete idea of the Tamil system of medicine which was in vogue till recent times in South India. The contents of the manuscript are in the form of stanzas so as to enable one to easily commit them to memory. This particular manuscript deals with cholera, vomiting, "Vayu" or wind trouble which brings about gas and flatulence in the abdomen, rheumatism, convulsions, and gonorrhoea and also lays down the prescriptions for them detailing the various Indian herbs and drugs to be used and also how these should be prepared and administered. For e. g. in regard to cholera, the recipe given is the following:—Turmeric powder to be fried in an earthen vessel; "Ganja" (an intoxicating drug) powder to be added and fried; twelve ounces of water to be poured and the frying to be continued till the whole water is evaporated by heating; the resultant material to be made out into small pills; the pill to be dissolved in pepper water and taken. The prescriptions given for various diseases include pills, powders, medicated oils, "kettue" (a peculiar preparation obtained by steaming), ointments, liniments and mixtures.

The latter part of the manuscript deals with environmental signs and indications auguring well or ill for the patients concerned (as having a bearing upon the prognosis of the case) e. g. moping of the owl from a particular direction, etc. Various religious rites and talismans are also suggested in regard to certain specific maladies.

The collection also included two delightful ivory sculptures, one representing an old Ayurvedic Physician and his rheumatic patient (Fig. 1) and the other an old Ayurvedic Snake Physician, his patient and his snake (Fig. 2).

H. E. S.